MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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Professor A. FOWLER, F.R.S., PRESIDENT, in the Chair.

Lieut. Stanley Fielder, O.B.E., R.N.R., F.R.Met.Soc., 5 Grandison Road, Clapham Common, S.W. 11;
Major E. O. Henrici, R.E., 29 Royal Crescent, London, W. 11;
William Porthouse, Municipal Officer, 27 Turnbull Road, Longsight, Manchester; and
William Stevens, Royal Observatory, Greenwich, and 17 Roxley Road, Lewisham, S.E. 13,

were balloted for and duly elected Fellows of the Society.

The following candidates were proposed for election as Fellows of the Society, the names of the proposers from personal knowledge being appended:—

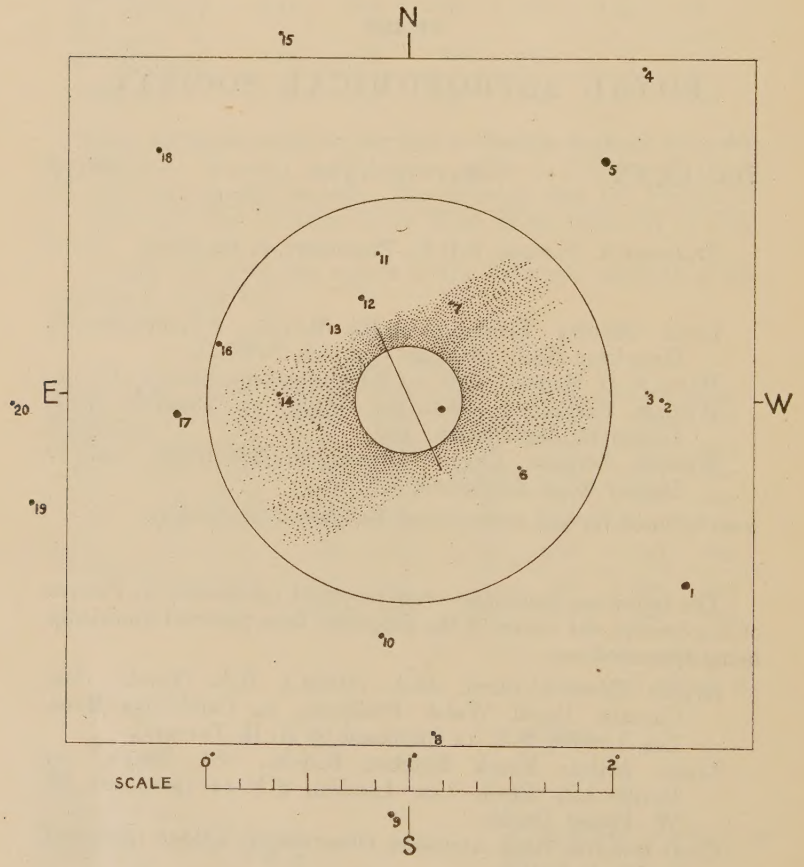
Arthur Edward Birch, M.A. (Oxon.), B.A. (Lond.) (late Captain, Royal Welsh Fusiliers), 24 Cambridge Road, Lee, London, S.E. 12 (proposed by H. H. Turner);
Lieut. Arthur Frank Butcher, R.N.R., "St. Mary's," 59 Burnt Ash Road, Lee, London, S.E. 12 (proposed by W. Fraser Doak);
Coert Hendrik Hins, Assistant, Observatory, Leiden (proposed by W. de Sitter);
James S. Lewis, Curator of Airdrie Observatory, Jesmond Dene, Victoria Place, Airdrie, Scotland (proposed by Hector Macpherson).

Eighty presents were announced as having been received since the last Ordinary Meeting, including, amongst others, a large number of publications from Institutions in Germany, Austria, etc.

The Solar Eclipse of 1922 *September* 20.

(*Communicated by the Astronomer Royal.*)

The results obtained by the expeditions sent to Principe and Sobral in 1919 show that the path of a ray of light is deflected when passing through the gravitational field of the sun. The



most reliable observations clearly indicate that the amount of the deflection conforms with that predicted by the Einstein Relativity Theory of Gravitation. The importance of the subject makes it desirable to repeat the observations with all possible refinement at the coming eclipse of 1922 September 20.

The field of stars near the sun at the time of totality is shown in the diagram, on which also is shown the position of the sun and the sun's axis. The scale is indicated by the circle drawn

at a radius of one degree from the sun's centre. The gravitational displacements of the stars are given in the accompanying table.

Stars near the Sun.

No.	No. in B.D.	Mag.	Distance.	Gravitational Displacement.
			′	″
1	+0° 2843	6·5	98	0·28
2	+1 2617	8·7	75	·37
3	+1 2618	9·2	70	·40
4	+3 2560	7·7	120	·23
5	+2 2489	3·3	92	·30
6	+1 2621	9·0	38	·73
7	+1 2623	8·9	31	·90
8	−0 2507	8·4	98	·28
9	−0 2510	7·8	122	·23
10	+0 2858	8·4	70	·40
11	+2 2493	8·5	44	·63
12	+1 2628	8·0	33	·84
13	+1 2629	9·0	33	·84
14	+1 2631	9·1	39	·71
15	+3 2576	8·2	115	·24
16	+1 2633	7·7	59	·47
17	+1 2636	6·2	69	·40
18	+2 2499	7·1	105	·27
19	+0 2880	8·0	116	·24
20	+1 2641	8·0	118	·24

The field was recently photographed with the astrographic telescope stopped down to 7 inches aperture, using Special Rapid plates, and the faintest stars on the diagram were well shown with a 30 seconds exposure. This exposure is not too long, and although stars 7 and 14 are likely to be lost in the corona, yet stars 6, 11, 12, and possibly 13, ought to be visible, especially as the minimum type of corona, with extension in the equatorial direction only, is to be expected. The outer stars present no difficulty, as they are comparatively bright.

Star 12 is photographically bright and may be registered with 10 seconds exposure, so that a useful result may be anticipated, whilst if stars 6 and 11 are also secured the determination would be nearly as strong as in the 1919 eclipse.

Although as a whole the expeditions of 1919 were successful, the partial failure with the astrographic telescope at Sobral is to be regretted. As stated elsewhere, this failure is attributed to the mirror of the cœlostat employed. In the plans for the future, therefore, it is designed to use this telescope equatorially mounted, avoiding the use of the mirror.

In a recent paper (*M.N.*, 80, 318, 1920 Jan.) an English equatorial form of mounting was proposed as suitable for the purpose, but after further consideration it was considered, as a matter of expediency, that it would be better to transport the complete astrographic equatorial to the selected station after adapting it to the appropriate latitude. To this end a new stand will be constructed, which, for transport facilities, will be made in four or five parts. The driving clock and other details will also have to be adapted, but these present no insuperable difficulties.

This proposal is mentioned in the hope that others possessing similar or other suitable telescopes may consider the possibility of adapting their equatorials.

As mentioned by Mr. Hinks at the March meeting of the Society, and printed in the *Observatory* for April, there are three convenient stations from which the eclipse may be observed:—

1. Bandidu, in the Maldive Islands (lat. 2° 56′ N., long. 73° 1′ E.), is on the central line with a duration of 4^m 10^s and an altitude of 34°, at which good photographs may be obtained. It has the advantage of occurring at 8^h 10^m a.m. local time, before the temperature has risen; there will consequently be little or no fall in temperature during the eclipse.

2. Christmas Island, in the Indian Ocean (lat. 10° 30′ S., long. 105° 43′ E.), although some distance from the central line, has a duration of 3^m 42^s, with an altitude of 78°. The eclipse occurs at noon.

3. The western coast of Australia is impossible, but Eastern Australia is favourable. Cunnamulla, in the interior (lat. 28° S., long. 146° E.), is accessible by railway. With a duration of 3^m 45^s and an altitude of 26°, good work might be done here.

The Eclipse Results and the Contraction of Photographic Images. Note by L. Silberstein, Ph.D.

Dr. F. E. Ross published last year, in vol. iii. of the *Abridged Scientific Publications from the Research Laboratory* (Rochester, Eastman Kodak Co., 1919, p. 128), some of his experimental results relating to the contractions of photographic images in their dependence on the developer. With tanning developers, as pyro-elon and caustic hydroquinone, the amount of contraction is by no means negligible; it ranges from 2 to 8 per cent. according to the diameter, and varies (exponentially) with the photographic density of the image. The effect takes place during the drying of the plate, the stresses responsible for it being due to the smaller water content of the tanned gelatine within and around the image, which therefore dries more quickly than its surroundings. Very interesting quantitative details and photomicrographs exhibiting the effect will be found in Dr. Ross's paper just quoted. Moreover, Dr.

C. E. K. Mees, the director of the said laboratory, gave a full account of Dr. Ross's investigations at the 28th April meeting of the British Astronomical Association, attended by many of the Fellows of the R.A.S. Thus further details need not detain us in the present note. My purpose is only to draw the attention of all who are interested in a critical interpretation of the Brazil results to the existence of this contraction effect, which in our case becomes particularly interesting in view of the presence of the image of the solar corona between (and even in the very field of) the images of the stars. As Dr. Ross has explicitly shown, such a factor will influence the stresses set up in all the surrounding gelatine. Of course, the effect sought for and observed on the Brazilian plates is of the nature of a dilatation rather than a contraction. But the Ross-effect gives at any rate considerable *distortions* (to use as general a term as possible) which cannot be left out of account. In this connection the large deviations of the star displacements from radiality which I have mentioned on previous occasions, and their somewhat irregular distribution around the sun, would also seem to call for some careful attention of all interested in an unbiassed testing of Einstein's theoretical predictions. I may add that Dr. Mees, who has kindly sent me a copy of his recent letter on this subject to Dr. Ross, is just making arrangements to check the possible influence of the said distortions upon the question of validity of the conclusions drawn by the Astronomer Royal and the British observers from the Brazil plates.

London:
1920 *May* 1.

Note (containing some of the remarks added at the R.A.S. meeting of 1920 May 14).—It would appear, from a subsequent conversation on the subject with Sir Frank Dyson, Mr. Davidson, and Dr. Mees, that the Ross-effect is almost negligible under the actual circumstances of the Sobral plates, especially with regard to the "attraction" exerted by the coronal image upon the star images on the gelatine. At the same time, we were all unanimous in accepting Sir Frank Dyson's suggestion that the Ross-effect will be of great importance in connection with photographs of double stars, owing to the small distance of the star images (and their correspondingly large mutual "attraction"), and, for the same reason, in connection with spectroscopic doublets, triplets, or more complicated groups of crowded spectrum lines. These, and perhaps some further points relating to photo-astronomical work, seem to deserve some special discussion.

The Parallax of Sirius by Photography.

By Oliver J. Lee, Ph.D.

The spectacular brightness of Sirius draws the attention and efforts of astronomers whatever their special fields. An excellent short account of the discoveries connected with Sirius is found in Aitken's book, *The Binary Stars*, p. 228. A summary of radial-velocity determinations up to 1905 is given by Campbell in *Lick Observatory Bulletin*, vol. iii. (1904–5), p. 80.

Direct photographs of the companion have been obtained by Van Maanen at Mount Wilson and by F. R. Sullivan at Yerkes.

The parallax of Sirius was one of the first sought for by the pioneers Henderson and Maclear. In *Memoirs of the Royal Astronomical Society*, vol. xi. (1839), p. 239, the former gives the value $0''{\cdot}23$ from 97 observations by himself and 67 by Maclear. These were made with the mural circle at the Cape of Good Hope. He says: "The error of this determination of the parallax may be estimated not to exceed a quarter of a second. . . . On the whole, we may conclude that the parallax of Sirius is not greater than a half a second of space, and that it is probably much less."

Since the days of Henderson and Maclear a carefully planned programme of observations was carried out by Gill and Elkin for the parallaxes of Sirius and half-a-dozen other stars. The following values were obtained for Sirius:

Gill with the 4-inch heliometer .	$+0''{\cdot}370 \pm 0''{\cdot}009$
Elkin with the same instrument .	$+0''{\cdot}407 \pm 0''{\cdot}018$
Gill with 7-inch heliometer . .	$+0''{\cdot}370 \pm 0''{\cdot}0097$

Up to the present time no determinations by photography have been published. The magnitude of Sirius is given as $-1{\cdot}58$ by Harvard, and the nearest set of five comparison stars obtainable in a 4–5 minute exposure range from $8{\cdot}2$ to $9{\cdot}3$, with a mean of $8{\cdot}7$. This difference in light of over ten magnitudes was not successfully overcome until the double rotating sector described in *Astrophysical Journal*, vol. xliv. (1916), p. 59, was put to the task. This sector was first used on 1916 March 8. The motor proved too weak, and it was soon replaced by a stronger one which gives us 7–8 revolutions of the sector per second, instead of the five we were getting with the old one. In a five-minute exposure there occur more than twenty "snap-shot" exposures, which are more than ample for obtaining a firm image of the star being occulted, when the definition afforded by the sky is fair or better.

Before reducing the fifteen plates from which the present parallax is derived, the measures were corrected for the orbital velocity of Sirius. The table given by Boss in the Preliminary General Catalogue was used. This table is based upon the orbit given by Zwiers in *Lick Observatory Bulletin*, vol. iii. (1905), p. 170, using the mass ratio $1 : 0{\cdot}393$.

The definitive parallax obtained is

$$+0''\cdot367 \pm 0''\cdot010.$$

The probable error corresponding to unit weight is $\pm 0''\cdot027$. Boss gives the proper motion in R.A. $-0^s\cdot0366$. This series of plates, covering a period of 2 years and 11 months, yields the value $-0^s\cdot0364$.

The details of observation and reduction will be given later in the *Publications of the Yerkes Observatory.*

Yerkes Observatory:
1920 *April* 24.

Analysis of the Proper Motions of the Reference Stars in the Greenwich Astrographic Catalogue. By Sir F. W. Dyson.

1. The proper motions of the stars in vol. iii. of the *Greenwich Astrographic Catalogue* have been determined by comparison with earlier catalogues, particularly with the catalogues of Lalande, Argelander, and the A.G. catalogues of Christiania, Dorpat, and Kazan. The positions of these stars for the epoch 1900 are very well determined, as in addition to the Greenwich photographic places there are meridian positions in the Greenwich, Christiania II., Berlin C., and Hamburg catalogues. Proper motions have been determined by Mr. Jones and Mr. Hollis, using as far as possible the computations of Schröter and Doubiago. The systematic corrections applied by these astronomers have been departed from in some cases.

The various catalogues employed have been, as far as possible, reduced to Boss's system, separate corrections being found for Argelander's and Lalande's zones.

Finally, the proper motions have been verified by direct comparison of plates about 20 years apart. The results will be given in vol. iv. of the *Greenwich Astrographic Catalogue.*

2. The spectral types have been kindly supplied by Miss Cannon. Analysis of the centennial proper motions of stars between 65° and 81° in the form $A + B \sin\alpha + C \cos\alpha$ gives

Type.	No.	Right Ascension.			Declination.		
			$\sin\alpha$	$\cos\alpha$		$\sin\alpha$	$\cos\alpha$
B and A_0	590	$+''\cdot19$	$-''\cdot33$	$+''\cdot96$	$-''\cdot28$	$-''\cdot42$	$-''\cdot23$
A_2–A_5	595	$+''\cdot15$	$-''\cdot58$	$+1''\cdot30$	$-''\cdot35$	$-''\cdot74$	$+''\cdot02$
F_0–F_5	672	$+''\cdot19$	$-''\cdot55$	$+2''\cdot78$	$-''\cdot95$	$-2''\cdot16$	$-''\cdot27$
F_8–G_0	489	$+''\cdot55$	$-''\cdot02$	$+3''\cdot31$	$-1''\cdot11$	$-2''\cdot84$	$-''\cdot11$
G_2–G_5	581	$+''\cdot56$	$-''\cdot02$	$+4''\cdot10$	$-''\cdot20$	$-3''\cdot07$	$+''\cdot45$
K_0–K_2	923	$+''\cdot01$	$-''\cdot38$	$+1''\cdot93$	$-''\cdot66$	$-1''\cdot15$	$+''\cdot03$
K_5–M	196	$+''\cdot43$	$+''\cdot27$	$+''\cdot89$	$-''\cdot52$	$-''\cdot46$	$+''\cdot24$

The constant term in the right ascensions is probably due to uncorrected magnitude equations. Apart from systematic errors, the remaining terms are made up of (1) correction to Struve-Peters precession, and (2) solar motion.

Precession would give

$$\Delta m \cos \delta_0 + \Delta n \sin \delta_0 \,.\, \sin \alpha \quad \text{to the R.A.'s}$$

and

$$\Delta n \cos \alpha \quad \text{to the Decs.}$$

where δ_0 is the mean declination of the zone, or about 71°, so that $\cos \delta_0 = \cdot 33$ and $\sin \delta_0 = \cdot 95$. The solar motion gives $x \sin \alpha - y \cos \alpha$ to the right ascensions, and $-z \cos \delta_0 + x \sin \delta_0 \,.\, \cos \alpha + y \sin \delta_0 \,.\, \sin \alpha$ to the declinations.

There is a systematic difference in the value of y as given by right ascensions and declinations:

R.A.	·96	1·30	2·78	3·31	4·10	1·93	·89
Dec.	·44	·78	2·27	2·98	3·23	1·21	·48
Diff.	+·52	+·52	+·51	+·33	+·87	+·72	+·41

No such difference was found in the proper motions in zones 24° to 32°, and this difference appears to point to a systematic error of about $''\cdot 50 \sin \alpha$ in the centennial proper motions of declination.

3. Examination of the systematic corrections applied to Argelander's zone observations did not throw any light on the question. Comparison was therefore made between the Greenwich astrographic and meridian places and Boss's Preliminary General Catalogue, as systematic error in the Greenwich Catalogues would have great effect on the proper motions. It was found that between declinations 65° and 81°

$$\text{Boss—Greenwich 1900} = +''\cdot 04 + ''\cdot 11 \sin \alpha + ''\cdot 08 \cos \alpha$$

and

$$\text{Boss—Astrographic 1900} = +''\cdot 06 + ''\cdot 13 \sin \alpha + ''\cdot 06 \cos \alpha.$$

If a correction $+''\cdot 12 \sin \alpha$ had been applied to the Greenwich declinations, this would have contributed about $+''\cdot 30 \sin \alpha$ to the centennial proper motions, and would have increased the discrepancy. The Greenwich 1900 Catalogue contains a very large number of observations both above and below pole. Analysis of the figures in the introduction to the catalogues gives

$$\text{Dec. (above—below pole)} + ''\cdot 13 - ''\cdot 03 \sin \alpha - ''\cdot 05 \cos \alpha.$$

This does not afford any ground for supposing recent Greenwich observations to have an error $+''\cdot 12 \sin \alpha$. One is therefore led to consider the possibility of a periodic error in the declinations and declination proper motions of the Provisional General Catalogue for the region 65°–81° N. dec.

A comparison of observations made at Greenwich between 1915 and 1918 gives for diff. of Dec. (Boss—Greenwich)

Hour.	Diff.	No.	Hour.	Diff.	No.	Hour.	Diff.	No.	Hour.	Diff.	No.
h	″		h	″		h	″		h	″	
0	+·16	5	6	+·26	6	12	+·15	9	18	−·31	10
1	+·29	9	7	+·02	6	13	+·04	6	19	−·18	7
2	+·15	6	8	+·17	10	14	−·38	7	20	−·08	8
3	+·36	6	9	+·17	5	15	+·06	6	21	+·06	7
4	+·07	5	10	+·05	8	16	−·03	8	22	+·08	7
5	−·05	6	11	+·15	5	17	−·14	7	23	+·03	8

On analysis this gives

$$\text{Boss—Greenwich} = +″·05 + ″·16 \sin \alpha + ″·04 \cos \alpha,$$

and supports the comparison of the P.G.C. with Greenwich 1900.

4. Comparison of Greenwich with the second Christiania Catalogue gives

$$\text{Gr. 1900—Chr. II.} = -″·12 - ″·03 \sin \alpha + ″·02 \cos \alpha,$$

while the comparison with Berlin C. gives

$$\text{Gr.—Ber. C.} = -″·01 - ″·09 \sin \alpha - ″·14 \cos \alpha.$$

Also, Mr. Jones has made a comparison of the results given by the Cookson floating telescope for the years 1911–18, and finds :—

$$\text{Boss—Cookson} = +″·04 + ″·08 \sin \alpha + ″·04 \cos \alpha$$
$$\text{Greenwich—Cookson} = -·02 - ·07 \sin \alpha + ·10 \cos \alpha.$$

This relates to declinations 40°–65° and not 65°–81°.

Also, as bearing on the subject, the proper motions in Boss's Catalogue for the region 65°–81° N. Dec. have been analysed, but the result is inconclusive, owing to the presence of stars with large proper motion. If all the stars be taken, we find

$$+″·18 + ″·51 \sin \alpha + 5″·08 \cos \alpha \quad \text{from R.A.}$$

and

$$-1″·94 - 4″·67 \sin \alpha - ″·23 \cos \alpha \quad \text{from Dec.}$$

giving accordance; but if σ Draconis, a star of very large proper motion, be omitted,

$$″·00 + ″·91 \sin \alpha + 4″·97 \cos \alpha \quad \text{from R.A.}$$
$$-1·22 - 3·25 \sin \alpha + ·13 \cos \alpha \quad \text{from Dec.},$$

showing a considerable discordance.

In conclusion, although one would hesitate to say that there is a periodic error in the declinations between 65°–80° N. of the Preliminary General Catalogue and other catalogues of a fundamental nature, there appears to be ground for further examination of this region of the sky.

Royal Observatory, Greenwich:
1920 *May* 5.

Comparison of the Proper Motions determined by Dr. Porter in the "Cincinnati Publications," No. 18, with those of the "Greenwich Catalogue" 1910 for the Zone 24° to 32° N. Dec.

By R. T. Cullen.

(*Communicated by the Astronomer Royal.*)

A catalogue of stars with large proper motions of more than 10″ per century has been published recently by Dr. Porter in the *Cincinnati Publications*, No. 18, part iv. It contains positions and proper motions of 3164 stars ranging from about the 6th to the 9th magnitude, distributed over a large part of the sky.

This catalogue arrived at Greenwich after the completion of the 1910 Catalogue (part ii.), in which are given proper motions for about 12,000 stars down to the 9th magnitude between the limits of 24° to 32° North declination.

The Cincinnati proper motions are obtained from almost every available source, and are based on the new Auwers system. No corrections have been applied for magnitude equation.

The Greenwich proper motions were determined from observations in the Greenwich, A.G., Bessel, and Lalande catalogues only, and are based on the Boss system. Magnitude equation was applied to the Greenwich and A.G. catalogue observations; also, any further errors depending on magnitude have been eliminated after the discussion of the proper motions for solar motion.

The mean differences of annual proper motion (Greenwich *minus* Cincinnati), corrected to Newcomb's precession, together with the mean discordance, have been arranged for each hour of R.A. in the following table:—

Greenwich – Cincinnati.

Limits of R.A.	Mean Difference of Proper Motion.		Mean Discordance.		Number of Stars.
	R.A. (s 0001).	Dec. (″·001).	R.A.	Dec.	
h h			s	″	
0–1	− 6	+ 7	±·0015	±·018	18
1–2	+ 3	+ 4	12	13	17
2–3	+ 5	− 1	8	15	12
3–4	+ 3	+ 7	18	12	15
4–5	− 7	+ 7	18	21	12
5–6	+10	+ 5	7	8	8
6–7	+11	− 2	13	15	8
7–8	+ 4	−10	13	21	14
8–9	+ 6	+ 7	9	6	8
9–10	+ 9	+ 1	16	18	9
10–11	+ 2	+ 8	9	13	20

Greenwich – Cincinnati—continued.

Limits of R.A.	Mean Difference of Proper Motion. R.A. (s·0001)	Dec. (″·001)	Mean Discordance. R.A.	Dec.	Number of Stars.
h h			s	″	
11–12	+ 2	+ 6	±·0012	±·010	12
12–13	+13	+ 6	13	15	18
13–14	0	+ 8	11	13	22
14–15	+ 3	+ 9	10	16	16
15–16	+ 5	+ 5	11	9	16
16–17	0	+ 7	11	14	12
17–18	+11	+ 9	14	9	10
18–19	+14	− 2	15	10	7
19–20	+21	+ 3	16	14	16
20–21	+15	+ 2	14	13	6
21–22	+ 2	+ 5	10	14	16
22–23	+ 4	+13	15	13	13
23–0	+ 7	0	±·0010	±·013	19
					324

Weighting each mean hourly difference according to the number of stars, the

Mean difference of P.M. in R.A. (Gr. – C.) gives +s·00048

,, ,, Dec. ,, ,, +″·0048

with a mean discordance of ±s·0013 and ±″·014 in R.A. and Dec. proper motion respectively, corresponding to a probable error of ±s·0011 and ±″·012.

If the mean hourly differences of proper motion be expressed in the following manner,

$$a \sin \alpha + b \cos \alpha,$$

with equal weights to each hour, we obtain

Greenwich – Cincinnati.

Difference of P.M. in R.A.	+s·00048	−s·00027 sin α	−s·00003 cos α
,, ,, Dec.	+″·0048	−″·0017 sin α	−″·0004 cos α

showing constant differences but extremely slight periodic differences between the two determinations.

As no magnitude corrections were used in the Cincinnati determinations, the differences of proper motion were also arranged according to magnitude, on the assumption that some explanation of the systematic difference might be due to the faint stars as against the bright.

Comparing stars of 8·0 magnitude and brighter with those fainter, we obtain

		$8^{m}\cdot0$ and brighter.	Fainter than $8^{m}\cdot0$.
Mean difference of P.M. in	R.A.	$+^{s}\cdot00043$	$+^{s}\cdot00054$
,,	,, Dec.	$+''\cdot0048$	$+''\cdot0037$

Thus showing that the systematic difference is common to both bright and faint stars.

The epoch of the most recent Cincinnati observations used in the proper-motion determinations being about the same as those for the Greenwich Catalogue, viz. 1911, an interesting comparison can be made for the epoch 1910, corrections for proper motion being applied in each case when necessary.

Taking

Greenwich minus *Cincinnati*.

Right Ascensions give	$-^{s}\cdot016$	$+^{s}\cdot001 \sin\alpha$	$+^{s}\cdot002 \cos\alpha$
Declinations ,,	$+''\cdot52$	$''\cdot00 \sin\alpha$	$''\cdot00 \cos\alpha$

A comparison with the Boss positions for stars in the same zone gives

Greenwich minus *Boss*.

Right Ascensions	$-^{s}\cdot003$	$-^{s}\cdot007 \sin\alpha$	$-^{s}\cdot001 \cos\alpha$
Declinations	$+''\cdot49$	$-''\cdot10 \sin\alpha$	$+''\cdot02 \cos\alpha$

the outstanding point being the apparent periodic error in the R.A.'s and declinations of Boss shown by both the Greenwich and Cincinnati observations.

The Greenwich R.A.'s being corrected for magnitude, a comparison according to magnitude gives an idea of the size of the magnitude equation in the Cincinnati R.A.'s, and in particular to the observer, as only one person appears to have made the observations.

Greenwich minus *Cincinnati*.

Mag.	R.A.	Dec.	No. of Stars.
	s	"	
6·0–6·9	+·003	+·41	34
7·0–7·9	−·003	+·58	86
8·0–8·4	−·020	+·47	88
8·5–8·9	−·025	+·50	94
9·0–	−·056	+·72	22

The Cincinnati positions depend upon 4 observations each, and those of Greenwich, 5.

The constant term in the R.A.'s is due to magnitude equation. In the declinations a systematic difference exists in the Greenwich observations as compared with those of Cincinnati and Boss.

The following is a list of stars with the most discordant proper motions, further observations of which would be useful to

improve the proper motions. The last columns give the most probable proper motion after an examination of the material supplied by the two determinations.

No. in Cincinnati Catalogue.	Mag.	1900. R.A.	1900. Dec.	Determined Proper Motion. R.A. C.	Determined Proper Motion. R.A. Gr.	Determined Proper Motion. Dec. C.	Determined Proper Motion. Dec. Gr.	Probable P.M. R.A.	Probable P.M. Dec.
		h m s	° ′	s	s	″	″	s	″
14	8·8	0 5 38	24 16	...	...	− ·121	− ·065	...	− ·065
82	7·9	0 35 0	26 7	..	...	− ·055	− ·007	...	− ·050
126	8·2	0 52 34	29 47	...	...	− ·079	− ·120	...	− ·095
205	8·8	1 27 43	28 45	+ ·0137	+ ·0194	...	...	+ ·0150	...
500	7·0	3 38 39	27 37	+ ·0125	+ ·0086	− ·178	− ·121	+ ·0107	− ·150
511	8·7	3 43 17	28 20	...	...	− ·325	− ·230	...	− ·230
608	8·0	4 37 22	27 30	...	...	− ·300	− ·244	...	− ·300
1112	7·5	9 19 31	30 56	+ ·0029	+ ·0076	...	...	+ ·0050	...
1561	8·6	12 19 32	31 50	− ·0207	− ·0152	...	...	− ·0180	...
2602	8·8	19 52 50	29 41	− ·001	+ ·0062	...	...	+ ·0026	...
3145	8·2	23 53 39	31 23	+ ·007	+ ·0033	...	...	+ ·0040	...

The above discordances are due in the main to the manner in which the observations of Lalande and Bessel have been treated when disagreeing with the more recent observations, and again when the material is scanty and the period over which the observations are made is very short.

As showing the difficulty in dealing with some of the old observations of Lalande and Bessel, the following table will explain some of the above discordant determinations :—

Corrections to Lalande and Bessel Observations.

Cincinnati No.	By Cincinnati.	By Greenwich.
82	+ 10″ to Bessel Dec.	No correction.
205	+ 1ˢ to one of two obs. of R.A. (Lalande).	,,
500	+ 1ˢ to Lalande R.A.	Lalande R.A. bracketed.
608	+ 10″ to Bessel Dec.	No correction.
3145	− 1ˢ to both Bessel's R.A.	Lalande R.A. bracketed.

In the Greenwich determinations corrections have been obtained to all zones between 24° to 32° N. Dec. for both Lalande's and Bessel's observations.

Note on the Changes of Period in the Variable Bailey No. 33 *in the Cluster M* 5. By H. H. Turner, D.Sc., F.R.S., Savilian Professor.

1. In the *Pub. Amer. Ast. Soc.*, 23rd meeting, p. 76, Professor Barnard gives a series of beautiful observations of this variable, showing that its period, though known to be close to $12^h\ 2^m\ 7^s{\cdot}300$, undergoes undoubted changes in the fractions of a second. He gives the average period determined by comparing each year's observations with the first year 1899: but I venture to think that both the nature of the changes and the wonderful accuracy of the observing may be exhibited more clearly by using a rather different method, and at his request I submit the following rearrangement of his material.

2. From 1899 to 1903 the period is $12^h\ 2^m\ 7^s{\cdot}293$. Let us adopt this for reference. From Barnard's table (*loc. cit.*) we can recover the error of epoch (obs. – calc.) referred to this period, as in Table I., if we multiply the number of elapsed periods shown in the second column by the difference of the average period from $7^s{\cdot}293$, as shown in the fourth column. The products (in the fifth column) are converted into minutes and tenths, as used by Barnard in his interesting paper on this star in *A.N.*, 4409.

3. Now it is clear from the magnitude of the residuals in column 5 that the period is not constant. Barnard has calculated the probable error of a normal place, and finds it $\pm 0^m{\cdot}46$; and this is probably an excessive estimate, since it is calculated on the hypothesis of a uniform period.

TABLE I.

Year.	No. of Periods.	Secs. of Average Period.	Excess over 7·293.	Error of Epoch.	C_2.	$O-C_2$.	C_3.	$O-C_3$.
				m	m	m	m	m
1899	0			0·0	−5·0	+5·0		
1900	688	7·293	0	0·0	−4·3	+4·3		
1903	2750	7·293	0	0·0	−2·2	+2·2		
1904	3671	7·279	−14	−0·9	−1·3	+0·4		
1908	6483	7·303	+10	+1·1	+1·5	−0·4	+22·7	−21·6
1911	8792	7·316	+23	+3·3	+3·8	−0·5	+14·4	−11·1
1913	10172	7·326	+33	+5·6	+5·2	+0·4	+ 9·4	− 3·8
1915	11566	7·314	+21	+4·1	+6·6	−2·5	+ 4·4	− 0·3
1916	12284	7·300	+ 7	+1·4	+7·3	−5·9	+ 1·9	− 0·5
1917	12992	7·293	0	0·0	+8·0	−8·0	− 0·7	+ 0·7
1918	13867	7·275	−18	−4·2			− 3·9	− 0·3
1919	14559	7·276	−17	−4·1			− 6·3	+ 2·2

4. Various hypotheses might be devised for representing the changes of period: here it will be assumed simply that there have been three abrupt changes: between 1903 and 4, between 1913 and 15, and between 1918 and 19. It is clear from the fifth column, which might be headed $O-C_1$, that the adopted period ceases to apply after 1903.

5. Under heading C_2 are given the epochs calculated from the formula

$$C_2 = +(N-5000)\times 0^m\cdot0010 \quad \text{or} \quad +0^s\cdot060,$$

where N is the number of periods in the second column. It will be seen that this formula fits the observations from 1904 to 1913 inclusive, but not those outside this interval.

6. Under heading C_3 is given the epoch calculated from the formula

$$C_3 = -(N-12800)\times 0^m\cdot0036 \quad \text{or} \quad -0^s\cdot216,$$

and C_3 fits the observations from 1915 to 1918 inclusive, but not those outside.

Hence the period was

1899 to 1903	$12^h\ 2^m\ 7^s\cdot293$
1904 to 1913	12 2 7 ·353
1915 to 1918	12 2 7 ·077

and the observations may be represented by three straight lines, excluding that of 1919, which starts a fourth line.

7. The first pair of straight lines does not intersect at a point intermediate between the dates to which they apply, so that when the period changes we must suppose also a slight change in phase. To avoid a change of phase when C_1 changes into C_2, we must make the change when $C_2 = C_1$, or when

$$(N-5000)\times 0^m\cdot010 = 0,$$

i.e. in 1906, when $N = 5000$.

We cannot do this without including 1904 in the first group, and though this is by no means impossible, this reason for upsetting the accordance of the previous three residuals scarcely seems adequate. When $C_2 = C_3$ we have

$$10(N-5000)+36(N-12800)=0,$$

or

$$N = 11100,$$

so that we can make the second change in period at this point without any change in phase.

8. There is another consideration in favour of putting the first change before 1904 rather than after. In discussing long-period variables it has been found that discontinuities (changes of period

or phase or both) are liable to recur at regular intervals. Now we can in the present case put the changes at the dates

$$1898{\cdot}5\,?,\ 1903{\cdot}5,\ (1908{\cdot}5),\ 1913{\cdot}5,\ 1918{\cdot}5,\ 1923{\cdot}5\,?$$

(using the decimal ·5 to indicate the date midway between the mean epochs of observation in two consecutive years), which make a regular series of the kind in question. There is no change at (1908·5), but it is a familiar experience with long-period variables that the opportunities for change are not always taken, so that this raises no objection. But if we put the first change after 1904, we cannot make a regular series at all, unless we carry on the first period to 1908·5, for which there seems scarcely sufficient warrant.

9. If we accept the grouping of Table I., then the numerical mean of the residuals (unit $0^m{\cdot}1$)

$$0,\ 0,\ 0,\ +4,\ -4,\ -5,\ +4,\ -3,\ -5,\ +7,\ -3$$

is $\pm 0^m{\cdot}32$, so that the probable error of a normal place is much less than the $\pm 0^m{\cdot}46$ mentioned in § 3, and may be put at about $\pm 0^m{\cdot}25$. To compare this variation in half a day with a long-period variable of, say, 300 days, we should multiply by 600, and the corresponding probable error would then be $\pm 2^h{\cdot}5$. We are accustomed in the latter case to errors of a couple of days rather than a couple of hours, and thus we can the more admire Barnard's wonderful accuracy in observation. If the observations can be continued we ought to get most valuable information, but of course there are few telescopes (and probably few observers) capable of watching a 14 mag. star in this efficient way.

[*Note added May* 26.—The question was raised at the meeting on May 14 whether the changes above noted might not be in some way due to the comparison star. But Barnard explains in *A.N.*, 4409, that comparison is made with this star twice, once in ascending to max., and again in descending from it, and that the interval between the comparisons is little more than an hour. Any change in the comparison star would thus be detected by a change in this interval, and is in any case sensibly eliminated from the determination of epoch. The observations provide, in fact, as a by-product, testimony to the constancy of the comparison star.]

Micrometrical Measures of Double Stars made in 1919.

By Rev. T. E. R. Phillips, M.A.

The following measures were made with the 8-inch refractor belonging to the Society :—

Star.	1920. R.A.	1920. Dec.	Mags.	Position Angle.	Dis- tance.	No. of Nights.	Date.
	h m	° ′		°	″		
Σ 3062	0 2·1	+58 0	6·9, 8·0	16·9	1·63	3	1919·712
OΣ 12 (λ Cassiop.)	0 27·3	54 5	5·6, 5·9	156·6	0·62	3	·734
Σ 60 (η Cassiop.)	0 44·0	57 24	4·0, 7·6	257·4	7·18	5	·702
73 (36 Androm.)	0 50·7	23 12	6·2, 6·8	48·7	0·81	3	·709
113 (42 Ceti)	1 15·2	− 0 58	6·2, 7·2	358·6	1·46	3	·823
180 (γ Arietis)	1 49·1	+18 54	4·2, 4·4	359·9	8·14	3	·757
186	1 51·7	1 27	7·2, 7·2	39·3	1·20	3	·890
202 (α Piscium)	1 57·9	2 23	2·8, 3·9	312·3	2·61	3	·736
OΣ 38 (γ² Androm.)	1 59·0	41 57	5·0, 6·2	111·1	0·59	5	·681
43	2 36·0	26 16	7·2, 8·8	32·6	0·86	3	·864
Σ 305	2 42·9	19 2	7·3, 8·2	314·3	3·24	3	·834
333 (ε Arietis)	2 54·6	21 2	5·7, 6·0	202·9	1·40	3	·777
OΣ 531	4 2·2	37 52	6·5, 8·2	116·3	1·26	3	·775
95	5 0·8	19 42	6·5, 7·0	320·6	0·89	2	·901
98 (14 Orionis)	5 3·5	8 24	6·0, 6·8	141·4	0·89	2	·901
Dawes 5 (η Orionis)	5 20·5	− 2 28	4, 5	79·6	1·33	2	·865
Σ 749	5 32·9	+26 53	7·1, 7·2	344·5	1·00	2	·814
OΣ 545 (θ Aurigæ)	5 54·3	37 13	3, 7·5	335·1	2·56	3	·687
Σ 1110 (Castor)	7 29·5	32 4	2·7, 3·7	215·7	4·74	4	·819
1126	7 35·8	5 24	7·2, 7·5	148·0	1·12	3	·256
1196 (ζ Cancri)	8 7·6	17 54	5·5, 6·2	285·2	0·93	5	·932
OΣ 215	10 12·0	18 9	6·5, 7	200·8	1·03	3	·262
Σ 1356 (ω Leonis)	9 24·2	9 25	6·2, 7·0	126·8	0·99	3	·264
1424 (γ Leonis)	10 15·5	20 15	2·0, 3·5	115·8	3·83	4	·235
1457	10 34·5	6 19	7·6, 8·5	321·5	1·52	3	·304
1536 (ι Leonis)	11 19·7	11 1	3·9, 7·1	38·2	1·80	3	·311
1670 (γ Virginis)	12 37·7	− 1 0	3·6, 3·6	321·5	5·65	3	·411
1687 (35 Comæ Ber.)	12 49·4	+21 41	5·2, 8·0	98·3	0·91	3	·393
1692 (Cor. Caroli)	12 52·3	38 45	3·2, 5·7	227·8	19·74	4	·470
1744 (Mizar)	13 20·7	55 20	2·1, 4·2	150·1	14·45	3	·481
1781	13 42·1	5 31	7·8, 8·2	289·5	0·82	2	·389

Star.	1920. R.A.	1920. Dec.	Mags.	Position Angle.	Distance.	No. of Nights.	Date.
	h m	° ′		°	″		
1865 (ζ Böotis)	14 37·3	14 4	3·5, 3·9	137·2	0·79	5	1919·257
1877 (ε Böotis)	14 41·5	27 21	3·0, 6·3	332·4	2·77	4	·390
1888 (ξ Böotis)	14 47·7	19 26	4·7, 6·6	70·6	2·29	5	·251
				70·1	2·47	5	·361
				71·0	2·48	5	·447
				70·5	2·51	5	·580
		Mean of all measures :—		70·6	2·44	20	·410
1937 (η Coronæ)	15 19·9	30 34	5·5, 6·0	81·5	0·68	3	·406
2055 (λ Ophiuchi)	16 26·9	2 10	4·0, 6·1	79·4	0·88	3	·411
2140 (α Herculis)	17 11·0	14 29	3·0, 6·1	109·8	4·46	3	·424
2272 (70 Ophiuchi)	18 1·4	2 31	4·1, 6·1	134·6	5·24	5	·416
				133·7	5·19	5	·599
		Mean of all measures :—		134·2	5·21	10	·508
OΣ 358	18 32·3	16 55	6·8, 7·2	186·2	2·15	3	·708
Σ 2382 (ϵ^1 Lyræ)	18 41·7	39 35	4·6, 6·3	9·5	2·94	3	·543
2383 (ϵ^2 Lyræ)	...	...	4·9, 5·2	119·0	2·51	4	·536
OΣ 369	19 8	71 57	7·0, 7·3	27·9	0·84	2	·694
Σ 2579 (δ Cygni)	19 42·5	44 55	3·0, 7·9	278·7	1·95	6	·705
2672	20 18·1	23 35	8·7, 8·8	301·2	0·71	4	·584
OΣ 413 (λ Cygni)	20 44·3	36 12	5·0, 6·3	48·1	0·63	3	·542
Σ 2799	21 25·6	10 44	6·6, 6·6	290·1	1·65	3	·795
2822	21 40·5	28 23	4·0, 5·0	135·2	1·27	4	·851
2909	22 24·7	0 26	4·0, 4·1	305·3	2·78	3	·628
OΣ 483 (52 Pegasi)	22 55·2	11 18	6·2, 7·7	235·6	0·92	4	·799

Notes.

Σ 3062. Components seem much more nearly equal than Struve's magnitudes suggest. Doubtful whether there is more than $0^m·5$ difference between them.

OΣ 38 (γ^2 Androm.). Angle slowly diminishing.

Σ 1196 (ζ Cancri, A.B.). Angle diminishing about 5° per year.

Σ 1670 (γ Virginis). From the measures made since 1911 it seems that this pair is now slowly closing.

Σ 1888 (ξ Böotis). In *M.N.*, **79**, 509, in the distance column the mean of all the measures should read 2″·27.

Σ 1937 (η Coronæ). Angle now increasing rapidly.

Σ 2579 (δ Cygni). The distance seems rather large, but the measures are very consistent *inter se*; and, taken in conjunction with those of recent years, suggest that the pair is widening again.

Headley, Epsom.

Observations of Minor Planets made with the 10-inch Refractor of the University Observatory, Utrecht, Holland. By J. van der Bilt.

Date.	Utrecht Mean Time.	App. R.A.	Parallax Factor.	App. Decl.	Parallax Factor.	No. of Obs. R.A.	No. of Obs. Decl.	Comp. Star.
1919	h m s	h m s		° ′ ″				
		(137) *Meliboea.*						
Sept. 20	11 25 7	20 15 59·88	9·314	− 2 42 57·5	0·851	4	2	1
Oct. 20	8 12 26	20 35 20·67	8·141	− 5 40 0·7	0·868	6	3	2
		(444) *Gyptis*, 9m·3 D.M.-scale.						
Sept. 29	12 10 39	0 59 37·76	8·461$_n$	+ 7 46 1·7	0·788	6	3	3
Oct. 2	12 54 17	0 57 33·58	8·793	+ 7 15 2·6	0·793	8	4	4
6	12 3 17	0 54 47·43	8·031	+ 6 33 49·9	0·797	20	5	5
11	10 49 55	0 51 20·07	8·722^a	+ 5 42 4·0	0·804	6	3	6
22	13 38 3	0 44 12·43	9·398	+ 3 51 25·6	0·824	6	3	7
		(584) *Semiramis.*						
Oct. 11	11 42 49	22 41 56·09	9·326	+15 1 2·3	0·744	20	5	8
20	10 9 31	22 42 4·77	9·109	+14 1 37·2	0·731	6	3	9
		(186) *Celuta.*						
Oct. 11	12 6 18	0 58 47·61	8·589	+ 7 22 6·7	0·791	10	5	10
21	9 40 24	0 47 19·14	9·038$_n$	+ 7 41 9·5	0·791	6	3	11
22	13 14 2	0 46 4·97	9·343	+ 7 43 22·7	0·799	6	3	11
		(216) *Cleopatra.*						
Oct. 20	8 39 25	20 29 8·99	9·266	− 4 26 45·5	0·861	6	3	12
		(89) *Julia.*						
Oct. 20	8 54 31	20 49 48·60	9·250	− 7 3 14·5	0·872	12	5	13
		(240) *Vanadis.*						
Oct. 20	10 36 57	0 13 51 25	8·428	− 2 24 32·4	0·854	12	3	14
22	12 54 27	0 12 34·32	9·372	− 2 31 29·5	0·852	6	3	14
		(82) *Alkmene.*						
Oct. 27	15 17 36	3 15 57·38	9·349	+19 37 22·9	0·704	6	3	15
29	12 54 24	3 14 20·43	8·185	+19 33 45·1	0·675	6	3	15
Nov. 1	14 25 57	3 11 37·12	9·266	+19 27 20·2	0·696	6	2	16

Date.	Utrecht Mean Time.	App. R.A.	Parallax Factor.	App. Decl.	Parallax Factor.	No. of Obs. R.A.	No. of Obs. Decl.	Comp. Star.
1919	h m s	h m s		° ′ ″				
		(44) *Nysa*, 9^{m}·0 D.M.-scale.						
Oct. 29	10 52 48	3 35 1·89	9·307$_{n}$	+12 37 59·0	0·762	8	4	17
Nov. 1	13 49 16	3 32 27·50	8·954	+12 25 25·1	0·760	6	3	17
7	11 7 18	3 27 7·04	9·082$_{n}$	+12 1 48·2	0·757	6	3	18
		(172) *Baucis.*						
Oct. 29	11 24 10	1 11 56·43	8·848	+24 6 26·1	0·618	6	3	19
Nov. 1	13 27 41	1 9 0·80	9·444	+23 51 19·4	0·681	6	3	20
		(5) *Astræa*, 9^{m}·3 D.M.-scale.						
Oct. 21	10 7 5	0 54 44·76	8·896$_{n}$	− 1 35 54·9	0·850	6	3	21
22	11 57 37	0 53 51·73	9·004	− 1 41 39·2	0·850	6	3	22
29	12 31 22	0 48 28·46	9·280	− 2 15 22·2	0·852	6	3	23
		(75) *Eurydice.*						
Nov. 18	11 58 51	4 1 35·93	8·401$_{n}$	+28 19 2·6	0·550	12	4	24
Dec. 15	14 9 2	3 34 20·05	9·553	+26 56 7·8	0·703	6	3	25
		(34) *Circe.*						
Nov. 19	13 58 56	4 17 42·86	9·166	...	...	4	...	26
Dec. 15	13 40 24	3 54 11·71	9·456	+12 36 6·4	0·779	6	3	27
		(92) *Undine.*						
Nov. 24	10 16 19	1 23 9·88	9·001	− 4 58 56·2	0·867	6	3	28
Dec. 10	9 8 21	1 20 31·65	8·983	− 4 1 7·7	0·862	6	3	29
15	10 56 13	1 20 48·16	9·424	− 3 35 46·5	0·854	6	3	30
		(129) *Antigone.*						
Nov. 24	10 33 47	2 11 54·47	8·716	− 3 54 46·3	0·862	6	3	31
		(106) *Dione.*						
Nov. 24	10 54 12	1 22 47·09	9·195	+ 5 23 25·7	0·812	6	3	32
Dec. 15	11 21 56	1 21 10·20	9·464	+ 6 9 0·7	0·817	6	3	33
		(121) *Hermione.*						
Nov. 24	11 28 32	3 42 23·93	7·606$_{n}$	+16 18 47·4	0·711	6	3	34
Dec. 15	13 23 56	3 27 7·32	9·475	+16 18 22·2	0·758	6	3	35

Comparison Stars.

Comp. Star.	Mean R.A.	Red. to Date.	Mean Decl.	Red. to Date.	Reference.
	h m s	s	° ′ ″	″	
1	20 16 18·34	+3·93	− 2 37 53·1	+22·4	A. G. Strb. 7013
2	20 33 39·79	+3·59	− 5 38 7·7	+23·0	,, ,, 7161
3	1 1 11·01	+4·49	+ 7 53 56·9	+26·1	,, Lpz.$_2$ 394
4	0 59 26·49	+4·52	+ 7 12 40·1	+26·6	,, ,, 383
5	0 54 7·27	+4·53	+ 6 24 25·5	+27·1	,, ,, 333
6	0 49 24·14	+4·53	+ 5 32 47·8	+27·4	,, ,, 304
7	0 45 33·49	+4·54	+ 3 50 35·7	+27·6	,, Alb. 211
8	22 42 1·93	+4·19	+15 9 37·9	+31·9	Bord. ph. Cl. 744, No. 11
9	22 40 10·43	+4·11	+14 5 50·7	+32·3	A. G. Lpz.$_1$ 9075
10	0 58 21·32	+4·57	+ 7 23 26·9	+26·9	,, Lpz.$_2$ 368
11	0 46 43·15	+4·59	+ 7 36 22·0	+27·9	,, ,, 285
12	20 28 15·84	+3·55	− 4 29 25·9	+22·5	,, Strb. 7119
13	20 49 39·38	+3·70	− 7 11 46·5	+22·8	,, Ott. 7486
14	0 14 9·61	+4·38	− 2 27 52·8	+28·0	,, Strb. 58
15	3 15 44·88	+5·12	+19 34 48·3	+15·7	,, Berl. A. 892
16	3 13 23·54	+5·19	+19 15 31·4	+16·2	Bo. VI. +19° 497
17	3 34 59·61	+4·92	+12 33 8·7	+14·1	A. G. Lpz.$_1$ 1064
18	3 27 58·03	+5·02	+12 5 21·6	+14·8	,, ,, 1036
19	1 12 18·54	+5·02	+24 4 48·8	+27·9	Pa. ph. Cl. 1598, No. 162
20	1 7 54·37	+5·00	+24 2 56·5	+28·6	A. G. Berl. B. 365
21	0 55 26·07	+4·50	− 1 37 52·4	+26·7	,, Nic. 188
22	0 55 27·31	+4·49	− 1 45 48·4	+26·6	,, Strb. 214
23	0 48 43·03	+4·49	− 2 20 24·5	+26·3	,, ,, 191
24	4 1 5·11	+5·81	+28 19 40·3	+10·2	,, Cbr. E. 1985
25	3 34 38·47	+5·88	+26 57 33·9	+15·2	,, ,, 1771
26	4 17 29·21	+5·24	...	...	,, Lpz.$_1$ 1274
27	3 53 36·45	+5·39	+12 32 13·7	+10·6	,, ,, 1151
28	1 23 43·37	+4·54	− 4 53 16·9	+19·2	,, Strb. 331
29	1 18 39·15	+4·36	− 4 4 32·4	+21·4	,, ,, 313
30	1 18 25·66	+4·31	− 3 38 5·0	+21·0	,, ,, 310
31	2 11 32·48	+4·63	− 3 46 52·3	+19·5	,, ,, 540
32	1 22 7·53	+4·62	+ 5 13 26·1	+24·8	,, Alb. 396
33	1 20 11·18	+4·48	+ 6 3 43·2	+23·2	,, Lpz.$_2$ 509
34	3 42 58·53	+5·37	+16 27 21·0	+12·8	,, Berl. A. 1010.
35	3 28 23·35	+5·43	+16 19 49·9	+14·3	,, ,, 945

Utrecht:
1920 *April* 26

Reception of W/T Time Signals at the Royal Observatory, Greenwich. By W. Bowyer.

(*Communicated by the Astronomer Royal.*)

Time signals, transmitted by wireless telegraphy, were first received at the Royal Observatory in 1912, following a request from the Paris Observatory with reference to the signals sent from the Eiffel Tower. For this purpose the Admiralty erected a short aerial and provided a two-circuit receiving set with crystal detector. In the early part of 1914 the possibility of automatic registration of the signals was considered, and apparatus consisting of Brown telephonic relays operating a syphon recorder was brought into use. By this method the telephone current was amplified by two telephonic relays, and a third relay, acting as interrupter, controlled the current flowing through the syphon recorder coil.

The advent of the war then stopped further work on automatic registration, and the only signals observed until late in 1919 were the Eiffel Tower time signals. The developments made in W/T receiving apparatus during the war having opened up the facility of employing wireless telegraphy for the determination of longitudes, it was considered desirable that the installation at Greenwich should be remodelled on a modern footing.

The advice of Captain J. Slee, R.N., was asked in the matter, and he was kind enough to give particulars of the plan to follow, and also undertook to procure valve-amplifying sets which would satisfactorily receive long-distance signals.

A new aerial was made and erected during 1919 August. The aerial is of the two-wire type, and is made of 7 strand No. 19 gauge phosphor-bronze wire, the separators being bamboo canes 3 feet long. It has a clear run of about 160 yards from the western corner of the new observatory to the octagon room roof, its highest point, about 70 feet from the ground, and a further 40 yards lead in to the wireless room. This aerial was put together at the observatory, and its erection was kindly undertaken by Mr. Bowen, civil engineer, R.N. College, Greenwich.

The amplifying apparatus employed consists of a Marconi seven-valve amplifier, M.D. type, covering a range from 600 to 5000 metres wave-length, and a seven-valve amplifier R.A.F. pattern covering a range from 4000 to 18,000 metres wave-length. In the Marconi amplifier the high-frequency amplification is carried out by valves of naval pattern "R 5," and rectification is accomplished by a "Q" valve, Marconi type. In the R.A.F. amplifier, valves of French pattern are employed throughout, three for high-frequency amplification, one to rectify, and the remaining three available for low-frequency amplification. The reception of C.W. signals is arranged for both sets by the use of the independent heterodyne method.

The receiving circuits were originally arranged personally by Captain Slee during November, who gave every assistance possible

in the initial stages of working the set. On his retirement from the position of chairman of the W/T Board, his successor, Colonel Wace, also gave highly valued assistance.

It was some time before any signals from a distance could be detected, and having heard rather imperfectly the Annapolis signal, it was considered that some attempt at an improvement should be undertaken. In this connection an appeal was made to Professor L. Fortescue of the R.N. College, Greenwich. He not only suggested rearrangements, etc., but was good enough to lend some apparatus and gave practical assistance which was invaluable.

Considerable time has been spent in the effort to improve signals, especially Annapolis, and the following notes on experience in this matter may be of interest. A slight improvement in working the two-circuit receiver was found by using coupling coils of larger diameter. Finally, coils of about eight inches diameter were employed. A further improvement was effected by altering the set to a three-circuit receiver, on the suggestion of Professor Fortescue. This change was made during February. In the circuit connected to the grid of the first amplifying valve it was found that increasing the inductance improved the signals. The three-circuit arrangement has been adhered to up to the present, and the following approximate particulars refer to the various circuits:—

Aerial.—Total length available about 260 yards, inclusive of the old aerial; capacity slightly over two jars; total tuning inductance 20,000 mic. henries, of which 5000 is placed in the coupling coil; variable condensers of a total capacity of 16·5 jars are placed in parallel with the tuning inductance.

Intermediate Circuit.—Total inductance of 20,000 mic. henries all placed in the coupling coils, and variable condensers of a total capacity of 16·5 jars.

Circuit to Grid of Valve.—Total inductance probably about 70,000 mic. henries, and variable condensers of a total capacity of 16·5 jars.

The *syntoniser* is designed to work with a variable condenser of 1·5 jars maximum capacity.

All coupling coils with the exception of that on the circuit connected to the grid are variable, so that inductance may be altered and the coupling varied by that method as well as by the usual sliding method.

For short waves a two-circuit receiving arrangement is frequently employed.

It is found that when receiving conditions are good a slight sharpening of signals is obtained by an increase of inductance with corresponding decrease in capacity, but when excessive interference occurs a slight improvement can be made by the opposite action. The station has unfortunately proved to be rather noisy when long-distance reception is attempted. In general, receiving conditions are better before noon than after, and the late afternoon is frequently very bad for interference. Signals become stronger

somewhile after sunset, but it is frequently well after midnight before there is appreciable decrease in atmospheric interference.

The long-distance signals attempted were Annapolis, Honolulu, and Darien. The first has been regularly received. Darien (10 a.m.) was received well on most mornings from the middle of January till the early part of March, when it became difficult and is now but seldom heard. The evening signal (18^h) from the same station has been heard on only a very few occasions. Many attempts have been made to detect the signals from Honolulu, especially during the week March 15–March 21, when special full-power signals were being sent out, but nothing up to the present has been heard. In addition, efforts have been made to detect the signal sent out from San Diego, California, but these have failed.

It may be observed in this connection that it is not known whether it is customary to send out time signals with the full power available at the sending station. The time signals now being regularly observed are those from Paris, Nauen, Lyons, Annapolis, and a constant watch is kept for Darien. The method usually employed in receiving signals is for two observers to take two chronometers to the receiving room and observe independently. Each observer compares the chronometer he uses with the mean time clock independently. To ascertain the degree of accuracy with which signals were received by the above method, attempts have been made to register the signals automatically by means of the apparatus mentioned earlier. The effort was not entirely successful, and only a few records have been made.

The construction of the long-wave amplifier which gives a permanent current through the telephones is not the best suited arrangement for a telephonic relay, which should be worked through a transformer. Other methods of recording signals by means of a syphon recorder have been used elsewhere, and it is hoped at an early date that it will be possible to obtain satisfactory registers of all time signals which are distinctly audible in the telephones.

Another method of testing the accuracy of signals received has also been employed, viz. the comparison of the time given by the Paris rhythmic signals sent at 10.30 a.m. with the time given by the ordinary time signals at 10.45, 47, and 49. By employing considerable amplification it is possible to make the signals audible to several observers at once, and the coincidence observations can be made with the sympathetic dial in use on the time department desk. This dial is also used in the regulation of the mean time clock, and is always used for the observation of ordinary time signals. The results obtained are shown on Table I. The times given are those derived from the use of the provisional clock error used on the day, for the usual time-signal service. It is not anticipated that these times will be altered by any considerable amount when the final reduction of all clock stars has been completed. On the whole, the agreement between the four observers is close, and the two sets of signals are in good accordance. It should also be noted that for the period April 21

to May 1 W.B. used the chronometer method previously referred to, which should avoid any lag in the telephonic relay action, as this observer was using a telephone directly connected to the valve amplifier.

Table II. shows the record of time signals received by the use of chronometers for the months of 1919 December, 1920 January and February. The times given are those obtained by using the clock error derived from the final reduction of all clock stars observed. It is intended to treat the later signals in the same way when the final reduction of star observations is completed.

It will be noticed that large differences occur at times between the Greenwich clock and the wireless time signal received, for example, the Paris signals during the first half of February. As frequent observations of clock stars were made during this period (see Table III.) and the clock rate was fairly constant, no explanation of this discordance can be offered. The same kind of discordance was found by Mr. Lewis when dealing with wireless time signals in the early days of their reception at Greenwich, and when the tapping method for transit observing was still employed. With the introduction of the travelling wire micrometer it was hoped that such differences would be much diminished if not entirely obviated.

Time signals received by wireless telegraphy at the Royal Observatory, Greenwich, from the beginning of 1916 will be published yearly in the annual volumes. The times given will be corrected for the differences between the provisional and final clock errors. Information will thus be available for further investigation at any future date.

[TABLE.

TABLE I.

Comparison of Observed Times of Greenwich Clock for Eiffel Tower Rhythmic Signals at $10^h\ 30^m$ *and Eiffel Tower Time Signals at* $10^h\ 45^m - 49$.

Obs. Date.	Rhythmic Signals at $10^h\ 30^m$. W.B.	W.S.	W.A.L.	G.R.	Time Signals at $10^h\ 45^m$ to $10^h\ 49^m$. W.B	W.S.	W.A.L.	G.R.
	s	s	s	s	s	s	s	s
April 7	59·96				59·97			
8	00·09				00·08			
9	00·08				·15			
12	·04		00·05		·05		00·05	
13	·13	00·22			·18	00·05		
14	·27	·28	·28		·25	·22	·28	
15	·12	·21	·18		·15	·10	·13	
16	·17	·21	·18	00·28	·22	·17	·22	00·22
17	00·12	·15			·10	·17		
19	59·95	59·98	59·97	59·98	·07	·05	·07	·07
20	00·01	00·03	00·03	00·02	59·98	·05	·03	·02
21	59·93	·05	·03	·04	00·05	·03	·02	·02
22	00·01	·00	59·98	59·99	·08	·02	·02	·02
23	59·92	·01	·94	·99	·00	·02	·03	·02
24	00·05	·04	00·02	00·03	·11	·03	·02	·03
26	·09	·05	·04	·06	·12	·03	·03	·02
27	59·97	·06	·06	·05	·05	·05	·05	·03
28	00·09	·15	·06	·15	·12	·08	·08	·08
29	·20	·16	·16	·25	·15	·13	·13	·18
30	·14	·07	·08	·08	·12	·07	·07	·07
May 1	00·00	·06	·05	·05	·10	·10	·03	·07

From April 21 to May 1 W.B. observed on a chronometer in the wireless room.

Mean difference, Time Signal minus Rhythmic Signal.

	s	
W.B.	+0·04	(21 days)
W.S.	−0·02	(17 days)
W.A.L.	+0·01	(16 days)
G.R.	−0·01	(13 days)

TABLE II.

W/T Time Signals received 1919 *December.*

Obs. Date. Dec.	Lyons, 9h. W.B. s		Paris, 10h. W.B. s	W.S. s	Paris, 10h 45m. W.B. s	W.S. s	Nauen, 12h. W.B. s	W.S. s	Annapolis, 17h. W.B. s
1									
2					00·24	00·21			
3					·31	·19			
4						·31			
5					·43	·27			
6					·20	·26	00·10		
7									
8					·14	·12			00·17
9	00·19				·05		00·05		·20
10					59·94	59·94	·00		·14
11							59·93		·14
12					·99	·96	·96		·10
13	59·99		59·87		·88	·99	·84		
14									
15					·91		·80		·09
16			·97		00·06		·78		
17			·94		·02		·85		
18			00·04		·07		·91		
19					·21	00·01		59·95	·12
20			59·97		59·95	·04	·95		·07
21									·10
22						59·96		59·82	
23						·90			
24						·95			
25									
26									
27								59·90	
28									00·04
29				59·99	00·01	59·99	59·76		59·99
30				00·12	·02	00·07	·80		00·01
31			00 07	·05	·05	·07	·81		·12

A signal in agreement with the Greenwich mean time clock is shown as 00·00.

TABLE II.—*Continued.*

W/T Time Signals received 1920 *January.*

	Lyons, 9h.		Darien, 10h.		Paris, 10h.		Paris, 10h 45m.		Nauen. 12h.		Annapolis, 17h.	
Obs.	W.B.	W.S.	W.B.	W.S.	W.B.	W.S.	W.B.	W.S.	W.B.	W.S.	W.B.	W.S.
Date.	s	s	s	s	s	s	s	s	s	s	s	s
Jan. 1							00·06	59·94			00·04	
2					00·15		·09	00·15			·14	
3							·05	·13	59·81		·22	
4											27	
5							·16		·93		·22	
6					·19		·27	·17	00·06		·19	00·30
7					·16		16	·15	59·85		·21	
8					12		·12	·11	00·29		·30	
9							·09	·20	00·92			00·25
10					08		·10	·03	59·94		·09	
11												
12			00 11				·17	·17			·20	·21
13			·22				22		00·19		·40	
14			·20				·22	·12	·25		·37	
15			·08				·15	·16	59·83		·13	·16
16			·06				·12	·03			·12	
17			·02				·25	·05	00·10		·13	
18											·02	
19			·30				·09	·05	00·08		·15	
20			·42		·15		·19	·12	·01		·29	·07
21					·11		·06	·11	59·86		·17	
22			·36		·12		14	·13	·96		·28	·18
23					·10		·14	·12	59·88			·17
24			·57		·23		·14	·11	00·09			
25												
26			·06		·10		·08	·05	59·92			
27			00·08		·22		·21	·16	·95		·10	
28	00·33		59·92		·25		·26	·23	·88		·11	
29			00·32		·17		·20	·18	·89		·14	
30			·45		·26		·28	·11			·19	·12
31	·18		·33		00·10		·10	·16	59 86		·13	

TABLE II.—*Continued.*

W/T Time Signals received 1920 *February.*

	Lyons, 9h.		Darien, 10h.		Paris, 10h.		Paris, 10h 45m.		Nauen, 12h.		Annapolis, 17h.	
Obs.	W.B.	W.S.	W.B.	W.S.	W.B.	W.S.	W.B.	W.S.	W.B.	W.S.	W.B.	W.S.
Date.	s	s	s	s	s	s	s	s	s	s	s	s
Feb. 1											00·25	
2			00·39		00·21		00·10	00·06	59·89		·17	00·06
3			·40		15		·17	·16	00·05		·13	·10
4			·59		·26		·20	·15	59·99		·15	·19
5			·63		·13		·16	·14				
6					·15		·13		·97			
7			·79		·19		·20	·13	00·01			
8												
9							·25	·18	59·96			
10			00·90		·35		·42	37				
11			01·06		·27		·33	·24				
12			00·42		·37		·33		95			
13			·82		·23		·25					
14			·71				·28	·14	00·07			
15												
16			·82		·19		·23	·10	59·90			
17			·64		·09		·12	·09	·92			
18			·69		59·92		·04	·05				
19			·35		00·04		·06	·04	59·91			
20			00·36		·06			·05				
21			59·23		·01		·14	·00	59·92			
22												
23			59·99		·01		59·99	59·87	·92		·13	
24			00·58		59·86		·90	·92	·86		·18	
25			·49		00·24		00·11	00·06	50·90			
26			·56		59·98		·02	·04			·03	
27			·30		00·07		·07	·09			·06	59·97
28			·50		00·21		·25		00·00		·10	

(The times given are not corrected for time of transmission of the signal.)

TABLE III.

Sidereal Standard Clock Errors observed in 1920 *February.*

Date.		Clock Slow.	No. of Stars.	Adopted Rate.
d	h	s		s
Feb. 2	7	8·49	7	+0·05
3	9	·51	10	·03
4	13	·52	7	·06
	22	·61	1	
5	13	·64	7	·08
6	9	·71	20	·10
	15	·72	4	
7	8	·81	18	·14
8	16	9·05	4	·10
9	8	·06	3	·05
10	18	·17	4	·04
11	8	·16	15	·06
	10	·18	4	
	21	·20	8	·08
	22	·27	2	
14	6	·35	2	·05
	9	·40	6	
16	2	·43	7	·06

Reception of Wireless Time Signals. By W. E. Cooke.

The Australian observatories are taking considerable interest just now in the reception of long-distance radio signals, for two reasons—

1. The proposed attempt to re-determine longitudes by direct comparison with Greenwich.
2. Professor Sampson's proposal to utilise a world signal for the determination of large celestial arcs.

We are naturally experiencing difficulties greater than those which appear now to have been overcome between comparatively close stations such as Paris–Washington, but in this communication I propose only to refer to the apparatus for the actual reception of rhythmic signals.

Two methods are under consideration—

1. That already described in *Monthly Notices, R.A.S.*, 1917 March.
2. Some form of photographic registration.

For brevity I shall refer to these as the "Cooke" and "photographic" methods. The latter has, so far, not been actually tried by us. It has been freely discussed, and the Commonwealth Radio Department in Melbourne intends to experiment with it. It seems to me to have two drawbacks—(1) its expense and want of portability; (2) the difficulty of sufficiently isolating the time signals. When we are listening to a European station, such as Lyons, we hear a world concert, which we cannot altogether eliminate. There are also the X's. We can tune so that Lyons is perfectly distinct to the ear, which is trained to disregard notes of different pitch, but I am afraid that any form of photographic registration which will be sufficiently sensitive to record Lyons or Annapolis will also record the other sounds and form a jumble. However, this is mere surmise. I mention it in order to explain why we in Sydney have been confining ourselves, so far, to the Cooke method, which requires only an easily constructed special relay.

This method is fully explained in the paper already mentioned, so it will suffice to state that the operator listens to a series of dots spaced about a second apart. These will occasionally stop for a period of the order of 10 or 20 seconds, and then recommence, and all that the operator has to do is to note the second by his clock when the ticks recommence. This marks the moment of coincidence of his own clock-beats with those of the transmitting station.

A practical test of this method was recently arranged. It was actually designed to ascertain the lag, if any, of a signal received at station A by wireless and transmitted by land line to another station B some miles distant. As the aerial of the Sydney Observatory is small and badly situated, Radio-Commander Cresswell thought it might be found necessary to utilise the Commonwealth Station at Pennant Hills, about 15 miles away, receiving the signals on their large aerial and relaying them along a land line to the Observatory. Several methods of relaying were tried, but eventually the two telephones (connected respectively with the wireless aerial and the Pennant Hills end of the land line) were placed mouth to mouth and lashed together. The signals used were those transmitted from the sidereal clock at Adelaide. They were received at the Sydney Observatory (1) through the Observatory aerial, (2) by relay from Pennant Hills. In each case the relay signal passed through a (separate) tongue of the special slow relay, which was actuated by a chronometer having a gaining rate of about 1^s in 50^s. The observers were W. E. Cooke and F. B. Cooke. Arrangements were made for four series, to allow the interchange of both observers and relay tongues, and thus eliminate both personal and mechanical equations. The first series failed on the Pennant Hills-Sydney section, but the other three were good; and in order to complete the full set, observers were interchanged about midway in the last series. There was a moderate amount of interference, but in the last series it was almost incessant, and made the test a severe one. Owing either to greater caution or less sensitivity in my own case,

I queried several of my observations at the time of writing them down. My son, however, felt no doubt about any of the figures he entered. We worked entirely independently of each other, and did not compare notes until after the completion of each series. The results, which are given below, seem to show—

1. That the method is a good one.
2. That there is no appreciable lag caused by relaying the signals over about 15 miles of land line.

Wireless Time Signals.

Lag between Pennant Hills and Sydney Observatory,
1920 *February* 20.

1st Set.				2nd Set.			
	Sydney direct. F.B.C.	via P.H. W.E.C.	Sy. – P.H.		Sydney. W.E.C.	P.H. F.B.C.	Sy. – P.H.
h m				h m			
2 58	23·5	...	0	3 24	32·5	31·5	+1
59	12·5	12·5	0	25	20·5	20·5	0
3 0	1·5	1·5	0	26	9·5	9·5	0
	51·5	51·5	0		58·5	58·5	0
	39·5	39·5	0		47·5	47·5	0
	28·5	28·5	0		36·5	36·5	0
	18·5	jam (19·5)?			25·5	25·5	0
	7·5	7·5	0		14·5	14·5	0
	55·5	feeble (54·5)?			3·5	3·5	0
	44·5	44·5	0		(53·5)?	52·5	
	33·5	33·5	0		41·5	41·5	0
	22·5	22·5	0		(30·5)?	30·5	
	11·5	jam (11·5)?			19·5	19·5	0
	1·5	1·5	0		8·5	8·5	0
					57·5	57·5	0
					46·5	46·5	0

4th Set, Relay Contacts interchanged and set divided into two portions.

	F.B.C.	W.E.C.			W.E.C.	F.B.C.	
h m				h m			
3 56	22·5	22·5	0	4 3	(43·5)?	43·5	
	11·5	11·5	0			32·5	
	1·5	(1·5)?			21·5	21·5	0
	49·5	(49·5 ?			10·5	10·5	0
	38·5	38·5	0		59·5	59·5	0
	27·5				48·5	48·5	0
	16·5	(16·5)?					
	5·5	5·5?					
	54·5	54·5?					

Sydney Observatory:
1920 *March* 19.

Determination of Longitude by Wireless Telegraphy. (*Geophysical Discussions*, 1920 *May* 7.) By Professor R. A. Sampson, F.R.S., Astronomer Royal for Scotland. (Plate 11.)

In opening what is, I believe, the final chapter in the long story of the determination of longitude, one cannot refuse a brief glance, in review, at its refractory history. We know how Aristotle said that Morocco was probably contiguous to India because there were elephants in both, and how the names of the West Indies and La Chine Rapids fix a mark on the expectations of their discoverers; but none the less the essentials of the problem were perfectly understood by early explorers of the globe, it was only methods of execution that defeated them. In Hakluyt's *Voyages*, what I take to be the first successful determination of a long arc is recorded. It was made in the Straits of Magellan, in 1578, by Mr. John Winter, a consort of Sir Francis Drake, in a "good and newe ship called the Elizabeth, of 80 tunnes in burthen"—the H.M.S. *Queen Elizabeth* of those days; it runs as follows:—

"The 15 of September the moone was there ecclipsed, and beganne to be darkened presently after the setting of the sunne, about 6 of the clocke at night, being then Equinoctial vernal in that countrey. The said ecclipse happened the 16 day in the morning before one of the clock in England, which is about sixe houres difference, agreeing to one quarter of the world, from the Meridian of England towards the West."

Both observation and calculation were evidently done in his head, so we must not cavil if he gave 90° W. in place of 70°, for his reasoning is sound in all its details.

John Winter was able to bring off his determination because he had the fortune to find a celestial time-signal common to Patagonia and England, in the form of an eclipse; but the problem of providing such signals, whenever and wherever they were wanted, has proved by its difficulty an extraordinarily active stimulus to research. The Lunar Theory, the Theory of Jupiter's Satellites, both owe a large part of their lives to it. The study of the Moon, once acclimatised for that purpose at Greenwich, has been carried far beyond the point where it interests navigators. We may connect with it the fact that the Admiralty are the publishers of Hansen's *Lunar Tables*. In another field we owe to the premium offered by them the effectual encouragement of the admirable school of British clock-makers of the eighteenth and early nineteenth centuries, leading to the evolution of that wonderful piece of craftsmanship, the marine chronometer. It is only to be regretted that the study of horology—which is no less than the laboratory study of the rotation of the Earth—has been left at its craftsman stage and has never been taken forward as a purely scientific interest, as that of the Moon has been.

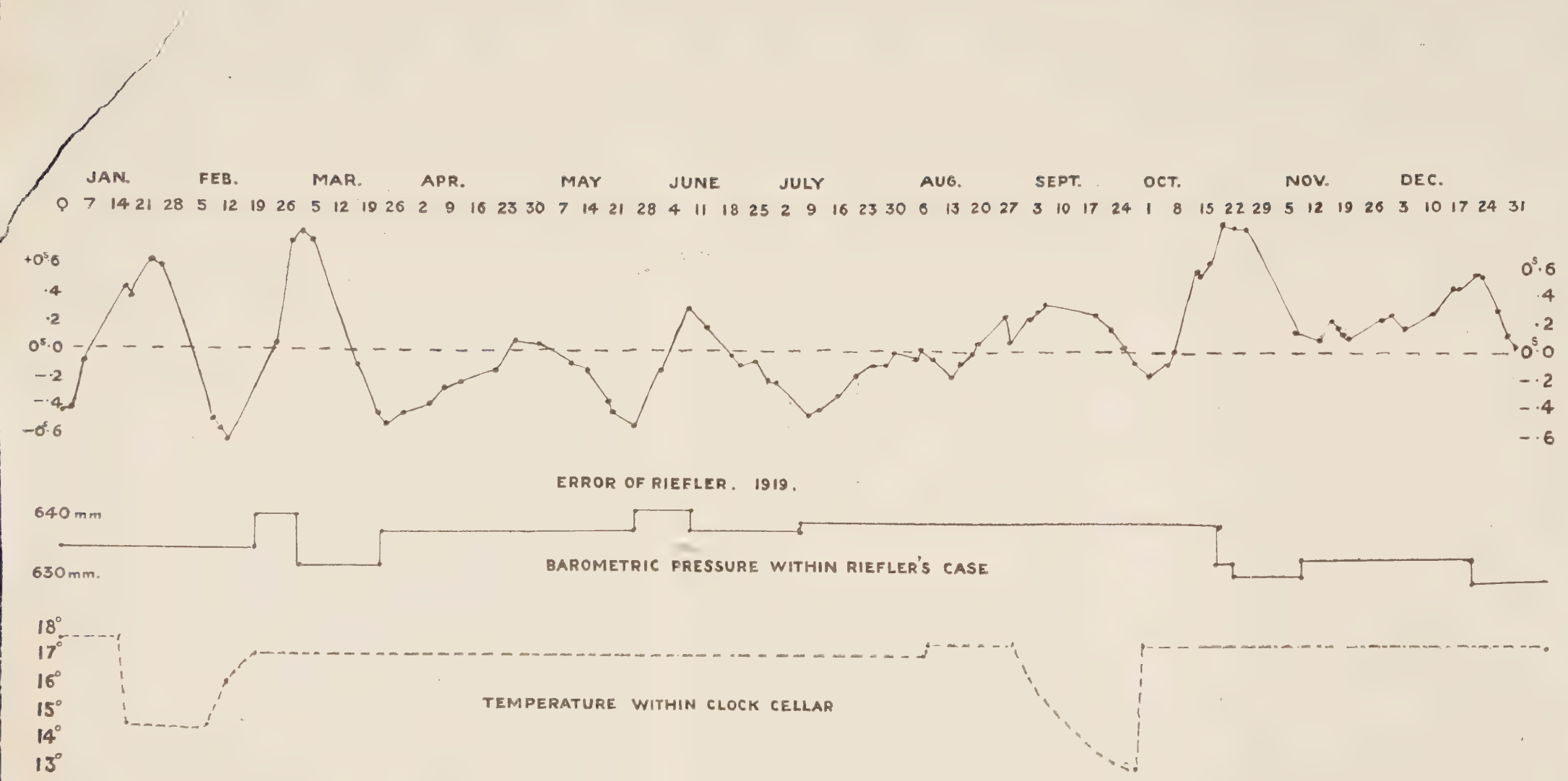
These remarks may seem discursive, but my purpose is to

show, to anyone who has a feeling for history, how deeply implicated the research of longitude has been in the progress of our knowledge of the Earth, and also of the sky. It should, of course, be recognised that to-day what we don't know about it ranks as an abstract scientific research. I recollect that on the conclusion of the last Paris-Greenwich longitude determination, a newspaper man who had become aware of it tried to convey it to his public as "settling a mere point of honour in pure mathematics." Whether the pure mathematician of to-day would describe it so, we need not ask. You take his meaning. If anyone supposes that there is any outcome of value, in the material sense, in sight in the research, he will be disappointed. As Burns says, "Let him turn and flee." We shall go forward without him.

Now, in my opening words I referred to the application of W.T. to longitude as the final chapter in its long history. By that I mean that we have in it an ideal answer to the search for a common signal. Longitude has lagged painfully behind latitude in its advance to knowledge because the sky supplied in profusion for the latter celestial marking points such as the Sun, visible to separated stations, from which the elevation of the pole could be derived. Now here we have at last a world signal available for the other co-ordinate also. We shall be dull indeed if we do not exploit it forthwith for all that it is worth.

What I propose to do in this address is to examine what such a project involves; our present position in respect to astronomical preparedness to undertake it; some indication of the instrumental position; an outline of what questions of scientific interest it involves; the organisation the research would require; and the co-operation outside of astronomical resources to which we must look, if any complete treatment is attempted. In this I shall confine myself to the problem of the long arc alone. And as I dislike to occupy any equivocal place, I should like to say that I approach the subject as an astronomer and have no pretensions to be an expert either in geodesy or in wireless telegraphy; I shall be grateful to those who are if they will correct or amplify any defect they may notice in my remarks.

We have heard a great deal recently about the nature of time as a fourth dimension, and so forth, but to the plain astronomer time is just another name for the phase of the Earth's rotation. The Earth's rotation is his standard of time, and his clocks are simply instrumental means of indicating it. If we marked Υ, $\aleph$ round the hours dial of his sidereal clock in place of 0^h, 2^h, the pointer should tell at any moment the star that is on his meridian. Now it is plain that in stellar observations, and largely in nearly all celestial observations of any kind, a reference to the Earth's phase of rotation only enters to be eliminated again as promptly as possible; and this, in fact, is what is done. Thus, chiefly by long-continued systematic massed observations at Greenwich, the system of R.A. of stars, round the whole circle, is known almost as


JAN. FEB. MAR. APR. MAY JUNE JULY AUG. SEPT. OCT. NOV. DEC.
0 7 14 21 28 5 12 19 26 5 12 19 26 2 9 16 23 30 7 14 21 28 4 11 18 25 2 9 16 23 30 6 13 20 27 3 10 17 24 1 8 15 22 29 5 12 19 26 3 10 17 24 31
+0s·6
·4
·2
0s·0
−·2
−·4
−0s·6
0s·6
·4
·2
0s·0
−·2
−·4
−·6
ERROR OF RIEFLER. 1919.
640 mm
630 mm.
BAROMETRIC PRESSURE WITHIN RIEFLER'S CASE
18°
17°
16°
15°
14°
13°
TEMPERATURE WITHIN CLOCK CELLAR

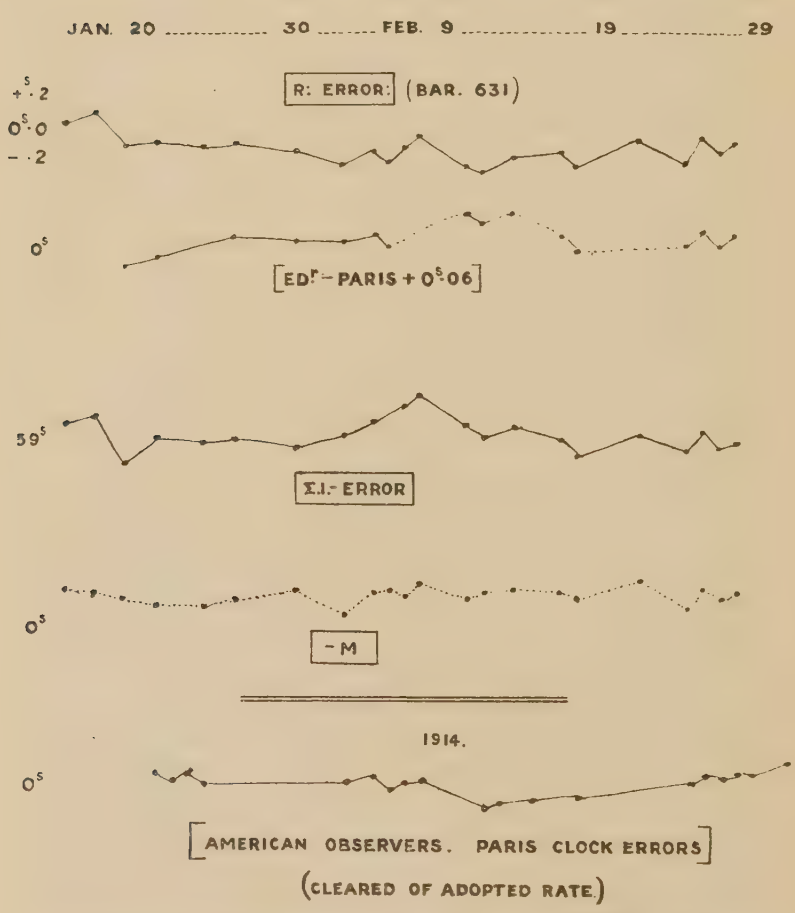
closely as we can observe the location of any special star between its immediate neighbours. It seems clear that there is no systematic distortion of the sphere of stars amounting to more than about $0^{s}\cdot02$ anywhere. The performance of the clock has been made a secondary matter. This is a marvellously fine result, but it should not content us. It belongs to the stellar region only. Wherever any bodies enter, like the Moon or Jupiter's Satellites, which themselves constitute a natural clock or alternative measure of time to the Earth's rotation, we are forced to incorporate the readings of the same clock at different moments, or those of separated clocks. In other words, the problems of instrumental time-keeping and of longitude are inevitable. It will not do for the astronomer to treat them merely as a branch of geodesy. So far as he is interested in fundamental work he must spare no effort to make those regions as exact and reliable as care can make them. Nor, if Greenwich is to remain in fact as well as in name the prime meridian, can we ignore the circumstance that the probable error attached to the Paris-Greenwich longitude is very much greater than that claimed for the Paris-Washington longitude recently executed by wireless telegraphy.

I want then to survey briefly the present position in its different directions, and first I would take that of time determination at a single station. In speaking of time-keeping I shall draw my remarks from two sources: the published account of the determination of the Washington-Paris longitude by wireless, and the experience of my own observatory at Edinburgh. It is now several years since I selected problems of time-keeping and the rotation of the Earth as suitable to the circumstances and equipment of the observatory. A good deal remains to be done, and much was interrupted by the war, but my equipment is fairly effective. I have a clock cellar of which the temperature is controlled automatically, a Riefler clock and two others, which, if not as constantly reliable as Riefler, are yet able often for spells of months together to compete fairly with it. I have given a good deal of care to the chronographic system. I should be the last to suggest that my observations are made with any more skill than elsewhere, but I think I can fairly say that the instrumental precautions are nearly as thorough as they can be made. I shall put a few figures before you; if they strike you as less accordant than you expect, I venture to say that you will not easily outdo them at the present moment.

The chart shows the run of Riefler's error for about a year. This error is generally kept between $\pm 0^{s}\cdot5$. When it reaches these limits it is brought back by the barometer. Reducing the indicated rates in runs of, say, twenty days or a month to standard barometer, say 630 mm., we are left with a slowly fluctuating base-rate running in this period from $-^{s}\cdot12$ to $+0^{s}\cdot03$, and known with hardly any uncertainty to $0^{s}\cdot01$ per day. This may be an oil effect, or something obscurely associated with temperature of the nature of stratification in the chamber, if it belongs to the clock at

all. Removing this base-rate, and remembering that there are one or two instances where the base-rate is caught changing, we find besides that we have for each observation a residual erratic feature. The mean of these erratics for Riefler is $\pm 0^s.05$.

I have also regularly taken for several years the scientific or



rhythmic time-signals of the Eiffel Tower, and thereby obtained a comparison of my clocks with those of the Observatory of Paris. These signals are arranged as a species of time-vernier; with a carefully arranged ticking attachment at the Observatory it is possible to fix the coincidences to a single beat, that is, to $0^s.02$. Recently these have been taken only on the nights when I had a determination of time, so that there was virtually no rate to carry

forward. The mean of these comparisons, now between 400 and 500, shows Edinburgh slow on Paris by $0^{s}\cdot06$, or the adopted longitude of Edinburgh $12^{m}\ 44^{s}\cdot2$ W.—too small by that amount. The erratic residuals outside this mean contain the faults in time determination at both observatories; their average is $\pm0^{s}\cdot07$, which would agree with an individual erratic feature in the French time-signal equal to that which I assign to the Edinburgh clock, provided this *erratic feature belongs to the observing system and not to the going of the clock.* For it is evident that it would only appear in the comparison of the two clocks if it were *incorrectly* ascribed to the clock error that is employed.

Hence it becomes a question of importance to obtain confirmatory evidence of the origin of this erratic feature. Taking for exhibition a run in the present year, January 15–February 27, when the determination of errors was very frequent and the erratics slightly more pronounced than the average and consequently the oscillations readily noticeable, by comparison with Paris, and also with the clock Synchronome I, cleared of a calculated portion of its rate so as to make its chart more speaking, it is pretty evident that the strongest features are common to all three, and therefore belong to the observations. Indeed, if further we chart the T.C. errors, it happens that we are able to assign a substantial part of the error to the quantity m which enters in the same way for all stars, and thus, if wrong, goes straight into the clock error.

The clock error is found with a hand-driven transit micrometer, and appears to be practically impersonal. Each is derived from nine or ten stars, with two or three polars; and the average discrepancy of each star from the indicated mean is much less than the established erratic element of the whole, being $\pm0^{s}\cdot02$ only.

Since the essence of longitude work is the correct determination of time, one naturally asks whether the anomalies unexplained above are escaped by other systems and other observers. Taking the Washington-Paris determination as an example of the scrupulous observance of precautions—in which, for instance, the azimuth of the azimuth marks, and the level, were read between every two observations and the instrument reversed in the middle of every observation,—we may say that they are not. The range of their individual night's observation is greater than mine; there still remain occasional complete obscurities as to what the clock is doing, of which I show a specimen. These erratic features the American observers attribute not to the clock but to the observations, though without more than tentative explanation.

I shall return a little later to the question of the value of elaborate precautions.

It is perhaps not the occasion to enter into further particulars. If, as I suppose, and as the Americans suggest, these erratics belong, not to the best modern clocks, but to the observations, a joint treatment of time by many observatories would be a powerful method for clearing them away.

I would make a remark upon the determination of time. It is, I think, sometimes dropped out of mind how many links there are in its chain. Speaking still of our practice at Edinburgh and starting from the stars, there may be catalogue errors; the geometrical errors of the instrument are certainly not easy to place beyond suspicion; there is conceivably a personal error with the observer, sending his signal to the chronograph; the chronograph is a fairly elaborate instrument, its zero depends upon the true return to its original set of a marker which pricks a paper fillet, and the unvaried movement of the armature of an electromagnet which starts from rest. We then start another chain before we get to the mean-time clock. Taking its pendulum as the real standard, its signals are made by a wheel on the scape-wheel arbor, and I have shown that with a micro-chronograph the inequalities of these wheels are measurable; this signal operates a P.O. relay, introducing an electromagnet with armature starting every second from rest; this in turn actuates a second pricker of the chronograph which is open to the same criticisms as the first. The time is taken by a coincidence of the two pricks, followed by a calculation based on the *Nautical Almanac*. The errors of such a system require examination. By setting another clock to give the signal in place of an observer, they can easily be estimated. I do this every week, taking a run of coincidences of M.T. and sidereal clocks each hour for nine consecutive hours. The average discrepancy from a run completely smooth numerically is $\pm^{s}\cdot010$, on the whole a somewhat surprising result, since an error of a single unit in marking the occurrence of coincidence would give half that amount, and there are two P.O. relays involved, working near their limit of operation, and finally the rounded *N.A.* figure may be anything up to $\pm^{s}\cdot005$ in error. But at any rate there is the chain, and if we are taking a wireless signal we must add to it the lag and accidental error of the apparatus that supplies the clock ticks—in my case the stroke of a little contact-making hammer agitated by the stroke of a pendulum controlled by Riefler's current. There are many of the links of the chain that might be called and are called, in different connections, the time; but none must be taken for another, and the problem of establishing their relative lags with security is fundamental to the research. It can be done, but only done, I think, with the oscillograph, as I have shown in numerous cases in a paper in *Monthly Notices*, 1918 June, on measurement of clock times to $0^{s}\cdot001$.

Turning now to the question of the correctness of the R.A. system, or absence of deformation in the reputed sphere of the stars. The method of using a common wireless signal at two widely separated stations has a bearing upon this that in my judgment should not be overlooked. If both observers take the common signal as their standard and at the same time observe what stars are on their meridian, this is exactly parallel to the method of testing a divided circle with two fixed microscopes.

By going over and over the circle systematically, the separation of the microscopes—that is, in this case, the longitude difference of the stations—and also the errors of division, viz. the systematic errors in R.A.'s of stars, will be certainly found. It may be that this method will not easily supersede the methods which have already yielded so much to patient application, but it is another and unused method, and it offers advantages that the other has not; it eliminates all reference to clock rate, and its determinations are cumulative in weight.

So much for the question on its side of technical astronomy. As to its features of technical radio-telegraphy, it is fortunately unnecessary for me to speak. Sir Henry Jackson is one of the earliest pioneers on the subject in its use in the Navy, and at the present date he is chairman of the Admiralty Radio Research Board. Besides that, General Ferrié, Director of Telegraphs of the French Army, whom I informed of our intention to hold this discussion, has most kindly contributed a paper which will be read presently. He has directed his remarks to the latest technical advances, which are great, very great indeed, since the Paris-Washington longitude was determined in 1913 with a crystal detector, a wave-length of 2200, and emission from Paris at 18 kw. To-day I am able to show you, by General Ferrié's kindness, the very perfect autographic records that are now taken at the Eiffel Tower, or by Professor H. Abraham, of various distant signals, including those from Arlington, Maryland.

My impression as an outsider regarding these rapid changes is that anyone who wishes to keep abreast of the latest interests must be prepared to scrap and replace his apparatus pretty frequently. And always, I suppose, at greater and greater expense, as the appliances get cleverer and more elaborated.

Let us now turn again to the goal of our efforts, the ideal way to use the world signal which W.T. provides—a true world signal even though we decide to use it like the Sun, only for half the world at a time. I take it the very heart of the project is a network, a chain embracing the whole globe, with a few long arcs, all determined with the same definite standard of precision, and providing an automatic check by the application of a closing error. If this were once done it might be improved upon afterwards, but from the beginning it could not fail to be the standard for reference of any other systems.

Let us make a rough survey of the present resources, and the minimum requisite for such a plan.

It may be taken at present that both emission and reception should be in the hours of darkness. Allowing some margin, this would point, say, to three main arcs AB, BC, CA, no one of which exceeded, say, 150°, and three emitting stations, S, T, U, between them. The arc AB would be found from emissions at S, BC from those of T, and CA from those of U. For this A, B, C must be observatories fully equipped for determining time and receiving radio signals, S, T, U must be powerful radio stations which can

be heard for at least one quarter round the world. There is no need for these to be provided with appliances for time determination of a high order.

If these conditions cannot be fulfilled, we must fall back upon more closely placed points, with the disadvantage that there are more elements to contribute errors.

Starting from the meridian of Greenwich (A), California (B) is 120° W., Western Australia is 120° E. and Victoria (C) 140° E. I learn from Mr. Cooke, Commonwealth Astronomer, that the Australian observatories are united in keen interest in this matter, and cannot doubt that the observatories of the Pacific Coast would be equally interested. Choosing stations from the map, we would put Annapolis (90° W.) at S to serve A and B, Honolulu (160° W.) at T to serve B and C, and say Mauritius (60° E.) at U to serve C and A. But in such matters any actual experience is of more weight than symmetrical designs from a map, and I am fortunate to have a very interesting letter from Mr. Cooke on the subject.

Independently of the present project, Mr. Cooke had formed a plan to link himself by wireless, eastward and westward from Sydney, with Greenwich, and had constructed two signal clocks for the purpose, sending them to Darien and Aden respectively, supposing these to be suitable stations.

However, he finds that with the best receiving set the Commonwealth Radio Service could supply, he hears Darien (100 kw., distance 8700 miles) very rarely. Honolulu (350 kw., distance 5000 miles) he receives unequally, and doubts if the time-signals are sent with full power. Certainly they are not seldom omitted. Aden proved unsuitable, as did also Cairo. On the other hand, Lyons in night transmission (distance 10,600 miles) can be read with the phones on the table ; and now, by the courtesy of General Ferrié, he is examining if time dots can be received. The 9 a.m. transmission from Lyons he cannot get. Cavite, Philippines (distance 3600 miles), he hears beautifully at all times.

There are, no doubt, capricious features in reception from different stations which hereafter technicians will clear up and probably remove for us.

Whatever the best solution may be, it is evident we are not very far from being able to organise a longitude net at the present day. It is perhaps idle to speculate further upon details, but I would suggest that we would naturally wish to duplicate the ties by adding a net of observatories in opposite latitudes, as the Cape (20° E.) to the Greenwich zone, Cordoba (60° W.) and Valparaiso (65° W.) to the Pacific zone, and Tokio (40° E.) to the Australian zone. One would thus surround the globe with a net of about six points roughly equidistant, the longitudes of which would be consistently determined with high accuracy.

What would such a scheme cost in time and money? Take the observatories first. It is a prime question with them. Their re-

sources are, as a rule, either small or are already occupied. From correspondence which I have had with members of the Commission de l'Heure, I gather that there is in some quarters fear that it might lead to an extensive paper programme, burdensome to the staff, disturbing routine, and not substantially advancing astronomy. Personally, my view is that if a joint treatment of the rotation of the Earth by the observatories of the world is possible, it is so important that it cannot be treated as a secondary problem, but must be organised alongside other matters even at the cost of some trouble and expense. But just how much would be the trouble and expense?

(1) It would require modern long-distance receiving apparatus, perhaps but not certainly, preferably, autographic.
(2) It would demand a thoroughly equipped time-keeping system, continuously studied, including all questions of chronographic lag and personality; but this should be considered a part of the system of any fundamental observatory, and not an additional impost.
(3) The actual receptions should, I think, be made at least on every occasion when time is determined, and when more trust can be placed in the clocks, on every day. The calculations are very short.
(4) There would be two stations to receive. Two half-hours a day might cover it, but these might be, one, say, at 8 p.m., and the other at 4 a.m. That is the only difficult feature.
(5) As to discussing the observations, usually an observatory would prefer to discuss its own, but it need not do so, and in any case they should be communicated to the Bureau de l'Heure, which should issue a joint discussion.

A governing question in settling the cost is whether it is essential to use identical duplicate instruments at different stations, and to interchange observers, in order to eliminate personality.

I would make some remarks upon this point. It has certainly been the custom in past longitude work to make every instrument in duplicate, and to reverse every step that can be reversed. This is, I think, carried to an extreme, *e.g.* in the Washington-Paris determination, where the instrument is reversed in its Y's in the middle of the transit of each star. This only abrogates the collimation error, and that at the cost of introducing a new "contact error" from the micrometer. But there is substantial evidence for doubt whether the advantages of such a plan outweigh its disadvantages. The latter are:—

(1) that the instruments must be small to be portable;
(2) they are not, in fact, identical; (*e.g.* the two levels in Washington-Paris example gave permanent differences);
(3) they are for exceptional use, and their behaviour and faults are therefore not so well known as the fixed prime instruments;

(4) the longitude expedition is a disturbing event, destroying the routine of habit and continuous work;
(5) and the requirements imply added cost.

Now we notice that the elimination of personality is not, in fact, secured. The interchange of American observers and instruments, end for end, Washington and Paris, changed the result by $0^{s}\cdot21$. Each set was beautifully accordant with itself, being (from plotted clock corrections)

$5^{h}\ 17^{m}$	$36^{s}\cdot549$	$\pm\cdot0051$
	$36^{s}\cdot758$	$\pm\cdot0027$

In the face of this discrepancy discussion is quite helpless. Of course the mean is adopted, but no better conjecture is given of the origin of their difference than diurnal variation of rate of the two Riefler clocks, which is not confidently prescribed, and is not sufficient, and, I venture to say, certainly does not exist.

But if we do not, in fact, gain what we aim at by this course, I suggest that we should try another one, viz. to build upon the very thorough study which the great fixed transit circles enjoy, organise the work so as to allow of continuous approach to our end, with cumulative evidence, and for the elimination of systematic error trust to variety and succession of normal observers, rather than transport men for short expeditions to unfamiliar circumstances. Among other advantages this plan would allow of execution without disturbing the regular work of the observatory or adding to its expense.

The work should, in my opinion, continue permanently, or at any rate for several years. A certain number of nights would be lost because there was no time observation at one or other of the two stations, and no reliable clock error carried forward. Besides, the most important conclusions would be cumulative; once the proper precautions were known and observed, they would tend speedily to become of irresistible weight.

As regards the radio stations, each would require the automatic clockwork for making the spaced dots, gaining, say, 1 in 50 seconds, to be put into operation at an approximately known but really arbitrary moment. This would be the only additional expense, not surely a material one to a station of the magnitude contemplated.

As regards the time required for emission, this would be, say, ten minutes once a day, in the neighbourhood of midnight, at an agreed-upon hour. No special technical staff would be required.

To sum up the position, what we stand to gain is:—

(1) a more exact knowledge of longitudes, the final settlement of the most obstinate of geodetical problems;
(2) consistency in time determination throughout the world;
(3) elimination of small remaining errors in systematic R.A. of stars;

(4) improvement in knowledge of clocks as timekeepers and as chronographic instruments;
(5) with some reaction to the benefit of radio research.

As to a practical proposition, it is understood that there will be a meeting of the Commission de l'Heure in 1922. These and cognate questions will probably supply its chief matter. There are some old international agreements as to ordinary wireless time-signals to rehandle, and one would expect a pretty strong international attendance when it meets. I would suggest that the British members should go to that meeting prepared with a well-thought-out plan, commensurate in scope with the responsibilities which our world position in all nautical matters implies. We should hope that the Admiralty would look with favour on this project. A great private company like the Marconi Company certainly could aid most powerfully, if we had the goodwill of their highly skilled staff, in advice and in keeping the observatories up to date in equipment. The commercial shipping interests of the country can hardly be said to be involved in any material way, but we hope that they would not be indifferent to its success, and would be able in some way to smooth its course. Upon astronomers the most responsible labour will fall. The consideration of perfecting their individual time systems is always before them. They might direct it with this end in view.

Note sur les Procédés actuels d'Emploi de la T.S.F. dans la Détermination des Longitudes. Par le général Ferrié, C.M.G., LL.D.

Le rôle de la T.S.F., dans la détermination des longitudes, est de fournir un signal instantané qui donne, pour chacune des stations dont on veut déterminer les différences de longitude, l'instant précis t auquel il faut noter l'heure de son garde-temps afin d'avoir, par différence avec les heures notées dans les autres stations, sa comparaison avec les autres garde-temps à cet instant. Son emploi est très avantageux, car il suffit que les stations considérées se trouvent dans le rayon de portée d'une même station radiotélégraphique, ce qui, maintenant, avec la multiplicité des grands postes de T.S.F. existants, se trouve à peu près réalisé pour des points quelconques à la surface du globe.

L'"état" E de chaque garde-temps sur le temps du lieu au même instant t est déterminé par ailleurs à l'aide d'observations astronomiques qui font connaître l'état E_0 à une époque t_0 voisine de t et la "marche" m, d'où l'on déduit

$$E = E_0 + m(t - t_0).$$

Quand on n'a pas besoin d'une précision supérieure au dixième de seconde, ce qui est le cas notamment dans la détermination du "point" à la mer ou en exploration, on se contente d'utiliser les

signaux radiotélégraphiques qui sont transmis à des heures connues du méridien de Greenwich. Le navigateur ou l'explorateur n'a qu'à déterminer l'heure locale à l'instant t d'un des signaux horaires qu'il perçoit dans le téléphone. Il note pour cela l'heure que marque son chronomètre à cet instant et il y ajoute l'état E calculé par la formule ci-dessus et partant du dernier état E_0 obtenu par les observations astronomiques. La marche m est alors fournie par la différence des heures marquées par le chronomètre à l'instant t et à celui du signal radiotélégraphique correspondant de la veille.

Si la précision désirée est au contraire très grande, par exemple de l'ordre du centième de seconde qu'il ne paraît pas utile de chercher à dépasser pour le moment, il devient nécessaire d'employer des procédés beaucoup plus exacts pour comparer les pendules aux signaux de T.S.F.

Les méthodes dont on peut faire usage sont au nombre de trois :—

1°. *Méthode des coïncidences.*—Le principe de la méthode est bien connu et il est inutile de le rappeler. Pour l'appliquer * on fait émettre par un grand poste de T.S.F. une série de points espacés entre eux d'un intervalle de temps égal à une seconde plus $\frac{1}{100}$ par exemple dans laquelle on supprime les points N°. 60, 120, etc., pour faciliter le comptage. Aux deux points géographiques dont on veut déterminer la différence de longitude on installe des récepteurs de T.S.F. dont le casque téléphonique est disposé de manière qu'il permette à l'observateur d'écouter cette série de points en même temps que les battements de son garde-temps. Celui-ci doit être muni pour cela soit d'un contact électrique se fermant chaque seconde, soit d'un microphone reproduisant le bruit du battement. L'observateur inscrit les heures marquées par le garde-temps à l'instant où se produit chacune des coïncidences entre les deux séries de battements, ainsi que le numéro des signaux de T.S.F. sur lesquels se produisent ces coïncidences. Avec un peu d'entraînement on arrive aisément à faire ces notations sans erreurs.

Il ne reste plus ensuite qu'à déterminer les diverses corrections nécessaires pour avoir l'heure locale exacte à l'instant du premier point (par exemple) de la série des signaux de T.S.F. Les détails du procédé opératoire sont indiqués dans le compte rendu de la Conférence Internationale de l'Heure de 1913.

Cette méthode, appliquée depuis 1910, a permis d'effectuer déjà un grand nombre de mesures de différences de longitudes et en particulier celle de Paris–Washington in 1913. Elle a été employée également pour déterminer de nombreuses positions géographiques dans le Nord de l'Afrique en utilisant les séries de signaux de T.S.F., semblables à ceux qui sont décrits plus haut, qui sont émis chaque nuit à 23 h. 30 m. par le poste émetteur du Bureau International de l'Heure (Tour Eiffel), et qui sont désignés sous le nom de Signaux horaires Scientifiques Internationaux.

* *C.R. de l'Académie des Sciences*, 7 février 1910, 21 novembre 1910, 1 mai 1911 (MM. Claude Driencourt et Ferrié).

2°. *Méthode des séries de même période.*—Si la série de signaux de T.S.F. dont il est question plus haut est émise de telle sorte que l'intervalle de temps qui sépare deux signaux consécutifs soit égale à une seconde de temps moyen exactement (par exemple comme dans les signaux transmis par les stations radiotélégraphiques américaines, notamment par Annapolis), on peut également l'utiliser pour comparer avec une précision de l'ordre du $\frac{1}{100}$ de seconde les pendules de temps moyen situées dans les stations dont on veut déterminer les différences de longitude.

Quand on écoute une telle série de signaux en même temps que les battements de la pendule locale, par le moyen indiqué précédemment, on perçoit nettement les deux séries, dont le son est généralement différent, et il s'agit de déterminer l'intervalle de temps qui sépare l'un quelconque des signaux de T.S.F. du battement de pendule qui le suit.

On a songé à faire usage de dispositifs qui retardent d'une quantité variable à volonté (depuis zéro jusqu'à une demi-seconde) l'instant où le téléphone est actionné par les courants produits par l'une des deux séries de battements (T.S.F. ou pendule locale), en intercalant par exemple quand c'est possible, un relais retardé dont le retard est réglable et mesurable. Ce procédé n'a pas encore donné de résultats vraiment pratiques.

Il a été proposé * également de passer par l'intermédiaire d'un petit moteur à vitesse rigoureusement constante et égale à un tour par seconde, pour évaluer le décalage des battements des deux séries en opérant de la manière suivante :

Un bras métallique est fixé à l'axe du moteur, dont il s'agit et vient toucher à chaque tour deux contacts métalliques distincts dont les positions peuvent être modifiées à volonté, et indépendamment l'une de l'autre, sur une circonférence concentrique à l'axe du moteur. On relie au bras métallique et à l'un des contacts mobiles un circuit électrique contenant une pile et le primaire d'une petite bobine d'induction. Le secondaire de celle-ci est intercalé dans un autre circuit contenant un casque téléphonique et le secondaire d'une deuxième bobine d'induction dont le primaire est relié à une pile et au contact électrique de la pendule locale (ou bien à un microphone placé de manière à recueillir les bruits des battements de la pendule). L'observateur entend donc simultanément, dans le casque téléphonique, la série des battements produits par la fermeture du courant entre le bras du moteur et le premier contact mobile, ainsi que la série des battements de la pendule. Il est aisé en déplaçant le 1° contact mobile de parvenir à superposer exactement les battements des deux séries de manière que l'oreille n'en perçoive qu'une seule.

Le bras du moteur et le 2° contact mobile sont reliés d'autre part à un autre circuit contenant encore une pile et le primaire d'une petite bobine d'induction dont le secondaire est intercalé dans le circuit du casque téléphonique du récepteur de T.S.F. On entend donc, dans ce casque, simultanément la série des battements

* *C.R. de l'Académie des Sciences*, 23 novembre 1912 (M. Lippmann).

produits par la fermeture du courant entre le bras du moteur et le 2° contact ainsi que la série des battements de T.S.F. Il est également aisé, en déplaçant le 2° contact, de parvenir à superposer les deux séries. Lorsque les signaux de T.S.F. sont constitués par des traits, on superpose les battements de la pendule aux débuts des traits.

Soit α l'écart angulaire en dégrés entre les positions des deux contacts après qu'on a effectué les deux opérations qui viennent d'être décrites ; la fraction de seconde θ qui sépare les signaux de T.S.F. des battements de la pendule est évidemment $\theta = \frac{\alpha}{360}$.

La difficulté d'application de cette méthode résulte de celle d'établir un moteur à vitesse rigoureusement constante et égale à un tour par seconde. On est cependant parvenu à synchroniser un tel moteur électrique au moyen d'une pendule. Si la vitesse n'est pas rigoureusement égale à un tour par seconde et a pour valeur $1 + \frac{1}{n}$ il est évident que l'erreur commise sur la valeur de θ est $\frac{N}{n}$, N désignant le nombre de secondes qui s'est écoulé entre les opérations de superposition des battements faites successivement avec les deux contacts mobiles.

Nous avons modifié cette méthode de manière à pouvoir faire simultanément les deux opérations et supprimer ainsi l'erreur $\frac{N}{n}$. Un moteur électrique est établi de façon que sa vitesse soit constante, autant qu'il est possible, et de valeur telle qu'un disque qu'il entraîne par engrenage démultiplié tourne à la vitesse d'un tour par seconde. Des résultats assez satisfaisants sont obtenus en faisant usage d'une magnéto motrice, actionné par accumulateurs et munis d'un régulateur de vitesse à force centrifuge, du type employé avec les appareils télégraphiques Baudot. La régularité de vitesse est encore meilleure avec une petite dynamo munie d'un régulateur spécial commandé par diapason (type Guéritot).

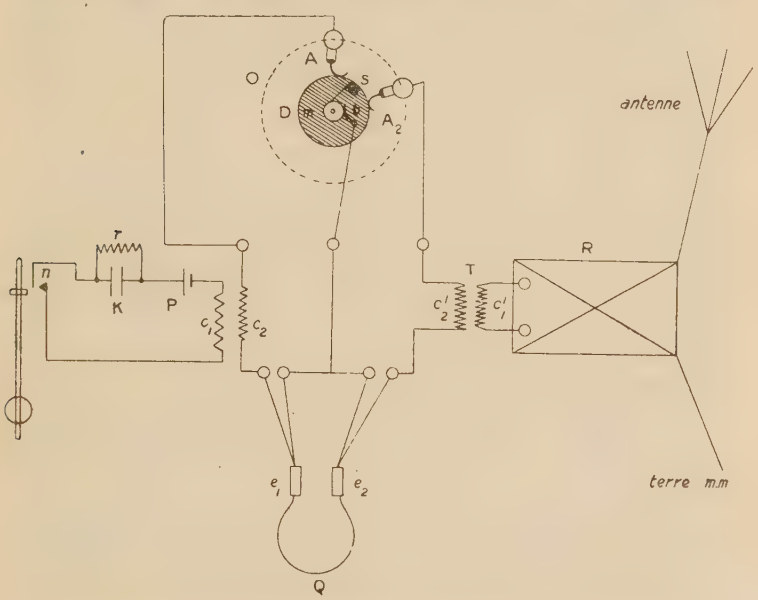
Le disque D entraîné par le moteur est fait en matière isolante, mais porte sur sa surface un secteur métallique s dont la largeur est égale au $\frac{1}{100}$ de la circonférence. Ce secteur s est relié à une bague métallique isolée m sur laquelle s'appuie un balai b. Deux contacts, munis de balais frottant sur la circonférence du disque D, peuvent être placés en des points quelconques d'une circonférence graduée O.

Le contact A et le balai b sont mis en circuit avec un des téléphones e d'un casque Q, et le secondaire c_2 d'une petite bobine d'induction I dont le primaire c_1 est relié à une pile p, à un condensateur K de 2 microfarads shunté par une résistance non inductive r ayant 30,000 ohms environ, et enfin au contact n, fermé par la pendule garde-temps P à chacune de ses oscillations. Le contact n peut être remplacé par un microphone ; on supprime dans ce cas le condensateur K et la résistance r.

Le contact A_2 et le balai b sont mis en circuit avec le 2°

téléphone e_2 du casque Q et avec le secondaire c'_2 d'un transformateur téléphonique T dont le primaire c'_1 est relié au récepteur amplificateur radiotélégraphique A. Celui-ci est connecté soit à l'antenne et à la terre, soit à un cadre récepteur fermé, établi suivant les dispositions actuellement employées en T.S.F.

Le moteur étant mis en route et réglé approximativement, on améliore le réglage de sa vitesse en écoutant dans l'écouteur e_1 les battements produits par les fermetures du contact n sous l'action des mouvements du balancier P. Pour que les courants produits par ces fermetures actionnant l'écouteur e_1, il est nécessaire que le contact n soit fermé par la pendule pendant que le balai A_1 passe



sur le secteur s. On cherche par tâtonnements la position de A_1 satisfaisant à cette condition, et, si la vitesse du moteur est bien constante et bien égale à un tour par seconde, on continue à entendre à chaque tour du disque le battement de la pendule P. En pratique on ne perçoit guère qu'un nombre de battements consécutifs compris entre 20 et 100. Mais il est facile de retrouver l'audition des battements, quand elle a disparu, en déplaçant légèrement à la main le contact A_1. Si l'on perçoit x battements sans toucher au contact A_1 cela montre que la vitesse du disque est supérieure ou inférieure à une seconde d'une quantité y égale au temps de contact du balai A_1 avec le secteur s, divisée par x. La largeur du secteur correspondant à $\frac{1}{100}$ de seconde, la quantité $y = \frac{s}{x}$ est de $\frac{1}{2000}$ dans le cas de 20 battements consécutifs perçus,

et de $\frac{1}{10,000}$ dans le cas de 100 battements. L'expérience a montré que les *variations* de la vitesse sont du même ordre quand le réglage et le montage du moteur, de son régulateur et des accessoires a été fait avec soin.

Ce réglage étant réalisé, l'opérateur fait le nécessaire pour entendre en permanence les battements de la pendule dans le téléphone e_1 et cherche à écouter en même temps dans le téléphone e_2 la série de signaux de T.S.F. recueillis par le récepteur radiotélégraphique F et qui sont produits avec une période égale à celle de la pendule P.

Il suffit à la rigueur que l'opérateur trouve par tâtonnements une position de A_2 qui lui permette d'entendre un signal entre deux battements ou inversement un battement entre deux signaux pour avoir la mesure du décalage des battements par rapport aux signaux. Si α est l'écart angulaire en degrés de A_1 et de A_2, le décalage exprimé en seconde de temps est $\theta = \frac{\alpha}{360}$. Les petits écarts de vitesse qui peuvent exister, comme on l'a vu plus haut, sont sans influence au degré de précision cherché. Les tâtonnements nécessaires pour trouver les positions convenables de A_1 et de A_2 sont notablement raccourcis quand on donne à s une assez grande largeur. Il est encore possible, même dans ce cas, d'obtenir une très grande précision en s'astreignant à placer les balais A_1 et A_2 dans les positions qu'ils devraient occuper si le secteur s se trouvait réduit à son commencement. Dans ces conditions, avec un peu de pratique, on parvient aisément à faire en peu de secondes une comparaison avec la précision du $\frac{1}{100}$ de seconde.

Il serait facile, si on en reconnaissait l'utilité, d'augmenter encore cette précision, mais il n'y a pas lieu de développer cette question pour le moment.

Le même dispositif s'applique évidemment pour la comparaison de deux pendules de même période. Il pourrait même être utilisé pour la comparaison de pendules de périodes différentes mais dans ce cas l'opération durerait plus longtemps. Il n'y a pas intérêt à exposer ici le procédé à employer.

La méthode qui vient d'être exposée est applicable aux comparaisons avec les signaux horaires américains. Les vides qui existent dans la série de ces signaux constituent des repères permettant d'obtenir la seconde h de la pendule qui suit un signal déterminé sans être obligé de compter les signaux. Il suffit, pour avoir l'heure de la pendule à l'instant de ce signal particulier, de retrancher de h le décalage θ pris dans le sens convenable.

Cette méthode n'a pas encore été mise en service réel.

3°. *Méthode de l'enregistrement.*—Les opérations que comporte la comparaison des battements d'une pendule avec des signaux de T.S.F. sont grandement facilitées si l'on peut faire usage d'appareils permettant d'enregistrer simultanément les deux espèces de battements sur une bande de papier, car il est alors aisé de mesurer avec la plus grande précision les écarts des battements des deux séries.

Cette méthode a déjà été employée, concurremment avec celle des coïncidences, pour déterminer en 1913 la différence de longitude entre Paris et Washington et elle a donné d'excellents résultats.*

La difficulté consiste a transformer les signaux hertziens reçus en courants assez intenses pour être enregistrés sans qu'il soit nécessaire de faire usage d'appareils intermédiaires susceptibles d'apporter des retards importants et variables.

La technique de la T.S.F., qui a fait d'énormes progrès pendant la guerre, permet actuellement d'enregistrer dans de très bonnes conditions des signaux de T.S.F., provenant de postes très éloignés, grâce à l'emploi des audions ou lampes à trois électrodes et à certains dispositifs spéciaux d'amplification des courants au moyen de ces appareils. On sait également réaliser un bonne protection de la réception des signaux contre des perturbations de toute nature.

La description de ces dispositifs radiotélégraphiques sortirait du cadre de cette note. Nous nous bornerons donc à des indications sommaires sur les appareils d'enregistrement proprement dits.

Les courants, issus des appareils récepteurs de T.S.F. et destinés d'ordinaire à agir sur les téléphones écouteurs, peuvent être envoyés directement dans un galvanomètre photographique de type quelconque sur la bande duquel ils s'enregistrent. Si on fait agir sur le même galvanomètre les courants produits par le contact de la pendule à comparer aux signaux de T.S.F., on aura sur la même bande l'enregistrement des deux espèces de signaux et il sera facile de mesurer les intervalles de temps qui les déparent. Pour augmenter la facilité de la mesure et éviter les erreurs pouvant provenir des irrégularités de la vitesse de déroulement du papier photographique, on inscrit encore sur la même bande les vibrations d'un diapason de fréquence connue. Les intervalles de temps à déterminer sont alors mesurés en vibrations et fraction de vibration du diapason.†

Ce procédé, qui a donné d'excellents résultats, est d'une pratique un peu délicate. Il y a intérêt à remplacer, chaque fois que cela est possible, l'enregistreur photographique par un enregistreur graphique.

On a fait usage en France avec succès, pendant la guerre, pour le repérage des canons par le son, d'enregistreurs comportant une bande étroite de papier noircie automatiquement au noir de fumée pendant son déroulement. Un électro-aimant, spécialement établi, est traversé par les courants à enregistrer. Sa palette porte une tige légère terminée par une fine plume d'acier. Celle-ci est appuyée sur le papier et les courants sont alors inscrits par un trace blanche sur le papier noirci.

L'appareil permet de faire tracer à la fois plusieurs plumes, commandées de la même façon que ci-dessus, sur la même bande de papier, et d'enregistrer ainsi simultanément plusieurs séries de

* *C.R. de l'Académie des Sciences*, 21 juillet 1913, M. Baillaud (méthode Abraham).

† Dispositifs, H. Abraham et A. Dufour.

signaux, par exemple : T.S.F., battement de pendule, diapason. Pour la T.S.F. et les battements il est nécessaire toutefois de tarer les enregistreurs, c'est-à-dire d'évaluer pour chacun d'eux le temps qui sépare l'instant où se produit le phénomène à enregistrer de celui où il s'enregistre effectivement sur la bande. On corrige de ces retards les lectures faites sur la bande.

Pour obtenir plus de sensibilité avec ce mode d'enregistrement on remplace l'électro portant la plume par un "magnéto-oscillographe à plume" qui permet d'inscrire très nettement des courants dont l'intensité est inférieure à un $\frac{1}{10}$ de milliampère.*

Cet enregistreur est associé à des appareils récepteurs de T.S.F. dont le pouvoir amplificateur permet de donner aux signaux à inscrire l'intensité suffisante pour actionner le magnéto-oscillographe.

La mesure des corrections à apporter aux lectures des signaux de T.S.F. et des battements de la pendule locale peut être évitée en enregistrant signaux et battements avec la même plume. Il suffit pour cela de disposer sur le contact électrique de la pendule un circuit constitué et placé de telle sorte que la petite étincelle qui se produit à chaque contact agisse sur l'antenne (ou le cadre) qui recueille les signaux de T.S F. à enregistrer. Les signaux produits par les battements de la pendule ont alors à actionner, avant d'être enregistrés, tous les mêmes appareils que les signaux de T.S.F. Les retards dûs aux intermédiaires sont ainsi rendus identiques et il n'y a plus de correction à faire. L'inconvénient de cette façon d'opérer est que la superposition de deux séries de signaux rend la lecture un peu plus difficile.

Les signaux de T.S.F. à transmettre pour effectuer des comparaisons de pendules par enregistrement peuvent évidemment être quelconques et très courts. Il suffit qu'on puisse les distinguer aisément.

Il n'est pas sans intérêt de remarquer incidemment que le même appareil enregistreur peut être utilisé comme "chronographe" pour les observations astronomiques destinées à la détermination de l'heure locale.

En *résumé*, on voit que, dans les déterminations de différences de longitudes, la partie de l'opération qui consiste dans la comparaison des pendules peut être faite par les diverses méthodes qui viennent d'être exposées avec toute la précision désirable, l'erreur restant inférieure à $\frac{1}{100}$ de seconde.

Les déterminations de différences de longitudes entre différents points de la terre effectuées jusqu'à ce jour sont très nombreuses. Celles qui sont adoptées ont été faites pour la plupart au moyen de lignes ou de câbles télégraphiques avec les instruments les plus divers et dans des conditions de précision fort inégales. Aucun plan d'ensemble n'avait été établi pour leur réalisation, de plus certaines d'entre elles sont manifestement erronées.

* *C.R. de l'Académie des Sciences*, 28 juillet 1919 (MM. H. Abraham et E. Bloch).

Il paraît donc nécessaire de profiter des grandes facilités que donne maintenant la télégraphie sans fil pour reprendre entièrement la question suivant un plan bien arrêté. Le point de départ de ce travail d'ensemble devrait être la détermination aussi exacte que possible des positions relatives de quelques points du globe qui formeraient le canevas fondamental auquel toutes les déterminations ultérieures seraient rapportées.

Le Bureau des Longitudes a pris l'initiative de consulter à ce sujet un certain nombre d'organisations officielles et de personnalités scientifiques. Les bases du projet présenté sont les suivantes :—

Constituer un polygone, fermé autour de la terre et comportant le plus petit nombre possible de sommets. Celui-ci peut être fixé à trois, situés dans l'hémisphère nord, sur des méridiens écartés de 8 heures les uns des autres de manière qu'il soit possible de faire des observations astronomiques simultanées en deux quelconques des trois sommets en opérant en automne ou en hiver.

Déterminer d'une part les latitudes des trois points et d'autre part les différences de longitude entre les sommets successifs en effectuant les comparaisons des pendules au moyen de signaux émis par des stations radiotélégraphiques puissantes et convenablement choisies. La somme des différences de longitude ainsi déterminées devant être de 360°, on aura une vérification qui donnera une idée assez nette de leur exactitude.

Les points proposés comme sommets du canevas principal sont Paris, Shangaï et San Francisco (ou un point situé dans le voisinage), dont les latitudes ne sont pas trop différentes.

Les signaux radiotélégraphiques seraient transmis par Lyon (ou Bordeaux) pour Paris et Shangaï, par Honolulu pour Shangaï et San Francisco, et par Annapolis pour San Francisco et Paris.

Quand cette opération fondamentale aura été achevée en mettant en œuvre les méthodes et appareils donnant le maximum de précision, il y aura lieu de relier par des opérations secondaires aux trois sommets du polygone principal, le plus grand nombre possible de points géographiques importants. Toutefois, il serait à désirer qu'on puisse profiter de l'exécution de l'opération fondamentale pour déterminer les positions géographiques exactes d'un certain numbre de points particulièrement importants. Il conviendrait notamment de comprendre dans les stations à déterminer *en premier lieu* l'Observatoire de Greenwich, de façon que les positions relatives de cet établissement et de celui de Paris, qui sont les deux plus anciens observatoires du monde et auxquels sont déjà rapportées un très grand nombre de longitudes, soient connues avec la plus grande exactitude et aussi que soient connues les longitudes absolues des points du canevas fondamental.

Il serait également très important de déterminer avec une très grande précision, la position géographique d'un point de la nouvelle Zélande, située à l'antipode. On connaîtrait ainsi avec une grande exactitude les positions relatives des quatre sommets d'un tétraèdre. En reprenant de temps en temps leur détermination on pourrait se

rendre compte de leur invariabilité au degré de précision des mesures.

Des études ont déjà été enterprises pour l'amélioration des instruments et des procédés susceptibles d'être employés dans l'opération fondamentale et aussi en vue de permettre de choisir judicieusement ceux qui devront être adoptés.

Il paraît nécessaire en effet de ne faire usage que de procédés et d'appareils partout identiques de manière à éliminer, le cas échéant, par la fermeture du polygone, certaines erreurs systématiques que pourraient comporter les procédés et instruments employés.

Les travaux entrepris dans ce but en France, sous les auspices du Bureau des Longitudes, ont permis de réaliser d'une part les enregistreurs très sensibles dont il a été question plus haut, et d'autre part un type d'astrolabe à prisme photographique * qui paraît susceptible de donner de bons résultats pour les déterminations dont il s'agit.

Une commission de savants appartenant aux nations spécialement intéressées à l'exécution de l'opération fondamentale (Grande Bretagne, Etats-Unis, France) pourra vraisemblablement être réunie dans un avenir pas trop éloigné pour arrêter le programme détaillé et définitif des travaux à entreprendre. La date de leur exécution pourrait être fixée à l'hiver 1921–1922.

En ce qui concerne le service international de l'heure, il se divise comme on le sait en deux parties distinctes:—Emission de signaux horaires à des heures fixées à l'avance et de telle sorte que ces signaux soient produits avec une précision de l'ordre du $\frac{1}{10}$ de seconde. Ce degré d'exactitude est largement suffisant pour les navigateurs, explorateurs, administrations, particuliers, etc. auxquels les signaux sont destinés.—Emission de signaux spéciaux devant permettre aux établissements scientifiques, et notamment aux observatoires, de recueillir l'heure internationale avec une précision de l'ordre du centième de seconde. Ces émissions présentent un grand intérêt pour les observatoires astronomiques en particulier, soit qu'elles leur évitent de déterminer eux-mêmes l'heure locale, soit qu'elles leur permettent de comparer le résultat de leurs observations à ceux du Bureau International. Cela suppose en revanche que leur longitude a été préalablement déterminée avec une grande exactitude.

Ces mêmes signaux présentent également l'avantage de permettre d'utiliser les pendules possédées par les observatoires qui les reçoivent, et éventuellement leurs observations d'heure a l'amélioration de la connaissance de l'heure par le Bureau International. Les résultats des comparaisons peuvent en effet être transmis chaque jour à ce Bureau, et entrer en ligne de compte pour la détermination des corrections à apporter à l'heure possédée par lui. Cette organisation est, en fait, pratiquée depuis la Conférence Internationale de 1912 qui a fixé le schéma des signaux

* *C.R. de l'Académie des Sciences*, 4 mai 1919 (M. R. Baillaud).

à transmettre (série de points comme il a été dit plus haut) et leur a donné le nom de "signaux horaires scientifiques."

Lorsque les observatoires intéressés seront munis d'enregistreurs du genre de ceux dont on a parlé au début de cette note, le service horaire sera grandement amélioré.

Des appareils de ce genre sont en cours d'installation à l'Observatoire de Paris (Bureau International de l'Heure). Ils permettront d'enregistrer régulièrement chaque jour les signaux horaires transmis par les grands postes radiotélégraphiques de Paris, Lyon, Nauen, Annapolis, etc., etc., et de contrôler effectivement l'exactitude relative de ces signaux.

Des spécimens d'enregistrements de ces signaux horaires sont joints à la présente note.*

Par ailleurs, l'existence de postes radiotélégraphiques de très grande puissance en Europe et aux Etats-Unis (Bordeaux et Annapolis) permettrait aux observatoires américains de prendre une part effective et efficace au service horaire international.

Il suffirait en effet que les postes de Bordeaux et d'Annapolis envoient chaque jour, à une heure déterminée, quelques signaux radiotélégraphiques qui seraient tous enregistrés à la fois au Bureau International de l'Heure (à Paris) et à l'Observatoire naval de Washington (chargé du service horaire américain). Les heures exactes du commencement du 1° signal de Bordeaux et du 1° signal d'Annapolis seraient déterminées dans les deux établissements, puis envoyées en même temps que les signaux du lendemain par le poste de Bordeaux, en ce qui concerne les déterminations faites par le Bureau International, et par le poste d'Annapolis, en ce qui concerne les déterminations du service américain.

Le même résultat pourrait aussi être obtenu en faisant transmettre par Bordeaux et Annapolis des signaux horaires scientifiques semblables à ceux que transmet actuellement la tour Eiffel.

Les heures correspondant aux signaux de la veille devaient être transmises avant les signaux eux-mêmes pour éviter toute confusion.

Ces émissions journalières pourraient également être recueillies par les observatoires du monde entier qui ont intérêt à profiter du service horaire international ou à y prendre part.

Il semble donc que l'on est bien près d'atteindre pour la détermination des positions géographiques et pour le service horaire la limite de précision que comporte l'état actuel de nos connaissances au sujet des positions d'étoiles. Pour aller plus loin encore il faudra améliorer les méthodes et appareils destinés à déterminer les positions d'étoiles. Peut-être conviendrait-il d'entrer dès maintenant dans cette voie.

Paris:
1920 *Avril* 22.

* [Not reproduced.]